

# Psychological Bulletin

## LANGUAGE AND PSYCHOLINGUISTICS: A REVIEW

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Because of the extreme pervasiveness of language, the literature in this field is not readily amenable to systematic classification. It is only necessary to point out that language phenomena have furnished data for such diversified approaches as are represented by the anthropologist, sociologist, philosopher, educator, historian, grammarian, philologist, linguist, physicist, physiologist, and psychologist (theoretical and clinical).

With such an ubiquitous attack directed upon the manifold aspects of linguistic phenomena, and with investigators motivated by highly specific interests, one may well expect trespassing of disciplinary boundaries and possible discrepancies in their statements about language; and, while the interest of the present paper is primarily in the behavioral phases of language events, it is nevertheless considered important to specify briefly the investigative techniques employed in certain parallel fields of inquiry in the belief that such a procedure of comparison and contrast may bring the essential psychological features of linguistic happenings into sharp focus.

### I. NON-PSYCHOLOGICAL STUDY OF LANGUAGE

*Anthropological Interest in Language.* The anthropologists' study of language is as broad as his investigation of other phases of group life, for example the presence or absence of certain skills, types of clothing, tools, ceremonies, et cetera. He may inquire into the problems of the distribution of linguistic families and the interrelationships of race, language and culture (cf. e.g., Sapir 152, pp. 142-154); or, as Kroeber (106, pp. 112 ff.) has done, he may observe the relative size of vocabularies of various anthropological groups, the characteristics of the sound combinations that they employ or the diffusion and rapidity of linguistic change. On the other hand, Malinowski's (120, pp. 296 ff.) field studies may be said to emphasize the specificity of verbal patterning and the impossibility of translation grammar in the comparison of linguistic responses among members of different cultural groups.

Henry (76) has shown the futility of inferring about the adequacy or inadequacy of a language as judged by its formal categories, illustrating the point with an analysis of linguistic expression among the Kaingang Indians of Brazil. By means of changes in pitch and force of articulation, through modification of sound patterning by fine movements of the pharynx, and by means of gross bodily and facial changes, the Kaingang achieves great richness of expression, though lacking in formal devices.

A final but, perhaps, more typically representative study of anthropological interest in language is the recent work of Harrington (72) on the American Indian sign languages. Comparison was made between this language and Chinese ideographic writing and its prevalence was traced to the area over which the buffalo persisted longest.

*Sociological Study of Language.* The sociologist's pursuit of language is brought out clearly in a review of recent Soviet studies in linguistics by Schlauch (153) who relates language research in the Soviet Union to the social divisions and struggles of those who speak them and linguistic change to political and social change, while a desire to learn about earlier social conditions motivates interest in linguistic paleontology and semantics through analysis of archaisms.

Sociological influences on the patterning of language are reflected in Hargan's (71) study of prison language and in Reinecke's (145) paper, "*Pidgin English*" in Hawaii: a local study in the sociology of language.

*The Philosophy of Language.* Since the work of philosophers is not limited by the rigours of field and laboratory investigations adhered to by the scientific disciplines, this very lack of restrictions is an inherent characteristic of the general approach to the philosophy of language and serves at once to segregate it from the sphere of activity engaged in by psychologists and other students of language.

The problems here concern such grand questions as "the relation between the structure of language and the structure of the world" (149, p. 265) or the relation of one sentence to another or the thing or things that the first sentence stands for or means. Cassirer (25) may be placed in this general tradition when he attempts to show how language makes possible an ordered world of spatial and temporal relations, and abstract concepts of substance and cause which would not exist without language. The works of Britton (15) and of Urban (179) also belong within this somewhat ill-defined framework.

An offshoot of the preceding line of work has been a long series of "meaning of meaning" books, beginning with Ogden and Richards' (134) exposition of that topic. Korzybski (105) and a succession of

popularizers [e.g., Stuart Chase (28), Hayakawa (74), Hugh Walpole (182) Aldous Huxley (86) and Irving Lee (112)], have treated the problem of meaning or semantics—the relationship of words and their referents to “clear thinking.”

*Educational Language Problems.* Foreign language study has stimulated a great deal of interest in the most economical methods of language learning. For example: is the oral method superior to the visual mode of presenting language materials? In answer to this question, Henmon (75) found that auditory presentation of digit, non-sense and sense materials was superior to visual, visual-auditory, and to visual-auditory-motor methods. Other experiments have attempted to find the optimal rate of learning, the most favorable length of, and interval between, practice periods or the most economical language unit for study. The effect of the native language upon the acquisition of another has been investigated by Garth and Smith (60), Arsenian (4), Mitchell (126), Stark (165), and Hackman (67) with somewhat contradictory conclusions regarding the benefit of a language supplementing the native tongue.

In another type of study, Grinstead (65) learned German by two different methods: (1) looking up formal lists and (2) reading for meaning and looking up only the unknown words. He found the contextual method superior to the word-list method both in immediate and in delayed recall. Briones (14) reached somewhat the same conclusions in a similar experiment in which he compared the learning of a series of isolated words with a series of phrases or sentences. The latter, as approximations to actual speech adjustments, were superior both in learning time and in retention tests. Besides its value in possible application, this finding has implications for language theory.

Still other aspects of language learning as they concern language teaching and study have been reviewed by Powers (142), Buchanan and MacPhee (16), Huse (85), Gray (64), Sharp and McDonald (170), and Fries and Traver (57).

While, as has been mentioned, experiments stimulated by educational problems may have significance for language theory, their primary emphasis is on the practical problem of language learning and not on the nature of language behavior *per se*. In this respect, language teachers share, in part, the enigmas of the arithmetic or English teacher on the best manner of presenting a course. At any rate, such studies serve to indicate the essential difference between the educator's and the psychologist's interest in language events.

*Experimental Phonetics*

*Physical or Acoustic Phonetics.* The physicist's participation in language study consists of abstracting one small portion of the linguistic field and proceeding to a physical analysis of speech sounds *qua* sounds.

The work of Scripture is illustrative of research in this line. As early as 1904, Scripture (154) recorded speech sound waves by means of the phonautograph, manometric flames, the kymograph and phonograph, and made harmonic and other analysis of these data. More recently, Scripture (157) has built a system of "atoms" and "molecules" based upon curves obtained from a simultaneous recording of sound waves and breath pressure. He considers the syllable as a "speech molecule" consisting of separate sounds as "speech atoms" but, despite an atomistic approach, Scripture (171) directs criticism toward phoneticians who

believe that the physical word consists of a series of speech sounds in which the gaps are filled with transition sounds termed "glides." All registration—macrophonic, microphonic and myokinetic—show, however that speech consists of continuous movements. There are no constant portions, no blocks of speech and no glides.\*

The more general physical characteristics of speech waves have been presented in a book by Harvey Fletcher (52), who in addition to using the usual instrumentation has perfected the oscillograph as an aid in handling certain sound engineering problems in the Bell Telephone Laboratories as well as a tool in dissecting the pitch, volume, resonance and other physical attributes of sound waves.

Another variety of the physical approach to speech has been reported by Paget (136) in his *Human Speech*, in which he analyzed and artificially produced voiced and unvoiced vowels by means of double resonators made of plasticene, and has applied these findings to a theory of voice production in the human during speech. He has also made a study of vowel resonance and of certain physical features of consonants.

By an oscillographic recording of speech sounds, physical interrelationships of sound patterns have been reported by Eliason and Davis (44). These investigators offer an experimental demonstration of the principle proposed by historical linguists that weak stress on the final syllable facilitates lengthening of the succeeding vowel and that the intervocalic consonantal explosion tends to shorten and disappear

\* Bloomfield (11, p. 76) is in agreement with Scripture on this point, for he writes: "Even a short speech is continuous: it consists of an unbroken succession of movements and sound waves. No matter into how many successive parts we break up our record for purposes of minute study, an even finer analysis is always conceivable. A speech utterance is what mathematicians call a *continuum*; it can be viewed as consisting of of any desired number of successive parts."



with decreasing stress on the final syllable. These results were connected up with certain historical changes in English. A final study by Herman (77) points up the possibilities of acoustic research; for it was a harmonic analysis of oscillographic records that permitted him to pick out certain wave differentia between hoarse and non-hoarse speakers.

*Physiological Phonetics.* The preceding account of phonetics has been restricted to the study of sound waves (linguistic physics); these sound waves are viewed as products or physical objects *produced* by a speaker, but it is also possible to probe into the sound-producing *movements* of the same speaker (linguistic physiology). The latter interest belongs to the division of physiological phonetics. Experimenters in this field have delved into the anatomico-physiological factors and such voice processes as respiration, phonation, resonance and articulation, with emphasis on the *functioning* of the speech organs.

Investigations have been carried out by means of devices such as kymographs, laryngoscopes and palatograms in an attempt to determine the exact position of tongue, soft palate, larynx, mandible and lips for the production of each speech sound.

This positional viewpoint early gave rise to a widespread belief in a fixed placement of the articulatory mechanisms in speech production, so that it was rather upsetting to phoneticians to learn of a case reported by Scripture (155) in which speech was effected without a larynx. A girl who had undergone tracheotomy at the age of three and was unable to expel *air* through the larynx could nevertheless speak distinctly but in a faint unvoiced manner. Phonautograph records showed intake of air at the beginning of speech, as in the normal speaker followed by a regular series of waves comparable to the vibrations of vocal cords in normal speakers. Further analysis showed that the "voiced" quality was produced by compressing the air in the pharynx and allowing it to escape between tongue and velum, this mechanism vibrating and functioning as an artificial glottis.

In this connection, it is interesting to mention Fletcher's (52, pp. 12-13) reports of tracheotomized persons who manage to speak well with an artificial larynx, showing that differentiation of speech sounds may be effected by mouth and lip positions also.

Another type of non-laryngeal voice production has been studied by Burger (19) and Kaiser (98). These investigators describe a subject who spoke with air from the stomach vibrating the mouth of the esophagus, producing "stomach speech" in this manner that had a range of two octaves. Besides this, Schilling and Binder (166, 167) report speech resulting from pharyngeal vibrations (i.e. buccal parts

above the larynx). A startling series of three cases has been described only recently by Goldstein (61); in these cases oral speech that was understandable and quite distinct as to articulatory nicety was developed after total amputation of the tongue. The author concluded that the tongue is not an indispensable organ of articulation; that other parts of the buccal cavity may replace its function; and that our knowledge about the physiology of the tongue as an organ of speech needs extensive revision.

Despite the argument of the above clinical and experimental findings against the fixed placement of speech organs, phoneticians have, until recently, taken an uncompromising stand on the assumption of a fixed tongue position for the production of vowels. In fact, the classification of vowels in the vowel triangle as accepted by the International Phonetic Association into "open" and "closed" is based on the distance of tongue bulge from the hard palate.

As the result of an elaborate series of experiments utilizing X-ray studies of tongue position, Russell (150) has shown the fallacy of the physiological vowel triangle. His X-ray pictures show no consistency for tongue position of any vowel from individual to individual. Indeed he adds:

It gave the author a distinct shock to discover that well trained *singers can sing on a loud swelling note almost any of these front vowels with the tongue relatively flat in the mouth*. Most vocal teachers would confirm this statement, and as a matter of fact, the majority of them spend considerable time in training the student to "keep the tongue out of the way" by so producing them (p. 153).

Since the physiological triangle conception of vowels is fallacious, Russell (150, p. 155) thinks it more valid to use an acoustic scheme to represent vowel facts.

Because the foregoing discussion of phonetics is likely to give the erroneous impression that there are two distinct camps in the field of experimental phonetics, it should be noted that there appears to be a recent eclectic trend toward the utilization of both physiological and physical speech data. Such an integrated viewpoint obviously including more features of the abstracted speech event than either alone is represented by Judson and Weaver (97) in their *Voice Science*, but is not entirely absent in earlier work in phonetics. Perhaps, the differences among workers in this regard may be said to be only a reflection of certain emphasis and preferences.

A more dynamic viewpoint of the physiological processes involved

in speech production is evidenced in the kinesiological approach of Stetson and his students, who stress movement of articulatory organs rather than their placement. The following quotation succinctly states Stetson's (166, p. 1) position:

The basic movement of speech is the pulse which produces the syllable. This syllable is a puff of air through the glottis made audible by the vocal cords in speaking aloud especially in the vowels, and stopped and started by the articulatory movements of the consonants. The larynx itself does not initiate the syllable nor control the process of articulation; this is quite apparent in the case of speech with an artificial larynx. The larynx makes possible the tone of speech. In just one thing is it a prime mover; it determines the pitch of the tone. The principal problems of the speech process is above and below the larynx (sic).

Stetson (166, p. 38) has substantiated his theoretical stand with experimental analysis. Action current records have shown the important functioning of chest and abdominal musculature in speech generally, while Stetson and Fuller (167) further demonstrated, by means of kymograph records that diphthong formation is a function of the rate of articulatory movements. Carrying the work along further, Hudgins and DiCarlo (82) have employed similar techniques in the analysis of the change in abutting consonants as determined by the rate of operation of physiological mechanisms.

It would not be amiss to indicate at this point, studies which, however, cannot be fitted into the classificatory scheme employed here. This is the work of Luckiesh and Moss (113) as reported in their book, *Reading as a Visual Task*. Aside from the important technical findings the work is interesting because it demonstrates (a) how thin a connection specific investigations may have with actual language phenomena and (b) the purely physical\* concern with factors in the situation under investigation. For example,

Certain physical characteristics of the paper and ink determine the absorption of the ink. This affects the sharpness and even the actual size of details such as letters. To some extent it affects the character of the surface of the ink; that is, its dullness or glossiness. Obviously the texture of the paper also determines the fidelity of reproduction of details (113, p. 206).

Such physical analysis as that of Luckiesh and Moss is farthest removed from the behavioral language datum although this does not deny that printed materials may enter into linguistic behavior situations, nor does it disparage the importance of such investigation.

\* This term is used consistently as an adjectival form of the noun, "physics," the scientific discipline and not in opposition to an alleged "spiritual."

*Phonology or Practical Phonetics.* The foregoing sketch of students of sound waves would exclude a large body of modern workers who are not satisfied to study speech sounds as "mere muscular movements or as disturbances in the air without regard to their use in communication" (11, p. 76). Their complaint is that laboratory phonetics does not enable us to connect speech sounds with meanings and unless we do resort to our everyday knowledge to tell us whether speech forms are the same or different, we cannot recognize the distinctive features of utterances on a purely phonetic level of observation. In consequence, they select as their unit of study, the phoneme, "a minimum unit of distinctive sound feature" (11, p. 79), which when synthesized into larger units yields the phonetic forms or structure of a language.

Workers in phonemics have devised a phonetic alphabet which consists of a distinct symbol for each phoneme of a given language. The investigator "hears out" the distinctive features in the speech of his informant and secures a transcription for further treatment. Analysis is restricted to transcriptions although phonograph records are also utilized. Techniques in this field have hardly been settled upon and disagreement among workers is common.

McDavid (117) urges the adoption of field recording in phonetic notation especially for dialect study, since microphones are likely to cause the informant some feeling of constraint. Besides phonograph records must be transcribed impressionistically in phonetic notation, and these transcriptions arranged and classified at additional labor and expense before they can be analyzed. A final argument against their use is the inferior recording of speech at the higher frequencies, in consequence of which McDavid urges the use of "the human ear" for both phonetic and phonemic notation.

Leaving the problem of method in this field, we progress to an examination of some current investigations in phonemics. Sebeok (158), for example, has constructed a phonemic system of Santali. Note that this investigator did not observe humans talking. His data were secured *in toto* from a grammar and a book of folk tales, an analysis of which permitted the determination, among other things, that "any vowel or diphthong may begin a word" and "that consonant sequences will occur within the word but never within the syllable" (158, p. 67).

Sebeok's study serves as an excellent illustration of the divergent interests between students of linguistic *behavior* and those of *phonemes*. The latter, handle, arrange and systematize textual materials; the former describe the action of an organism typically talking *to someone about something*. The same characterization applies to Haas' (66) study,

in which she analyzed types of reduplication in Thai derived from sounds which they were meant to imitate (e.g., *tum tum* for the sound of a drum, et cetera). Hockett (79) has recently offered a classification of English verb forms and verb phrases, and Trager (176) a historical phonology of the Tiwa languages. A slightly philological study is that of Hall (70) who ascribes the origin of the Romance words for "flute" to cultural borrowings rather than onomatopoeic origin.

An elaborate analysis of Hidatsa texts collected by Lowie in 1911 has been recently analyzed by Harris and Voegelin (73), and Voegelin (180) has painstakingly worked out a classification of Shawnee stems.

All in all, such investigations as students of phonology have brought forth involve them in textual or transcriptional analysis. Kantor (101, pp. 46 ff.) has termed these abstracted materials "thing language." This is not to criticize interest in such specialized portions of the linguistic event but only to distinguish again the behavioral datum from other aspects of language phenomena.

## II. PSYCHOLOGICAL STUDIES IN LANGUAGE

### *Experiments Utilizing Language-Derived Materials as Stimuli*

Only brief mention need be made of a long line of experiments that began when investigators started to introduce verbal materials as stimuli. Cattell's (26) thorough study of reaction time was a pioneer attempt to use words as stimuli. Among other results, Cattell showed that reaction time to words was very little longer than that to single letters and that speed of reaction is also a function of familiarity of the printed material.

It was at about this time that verbal materials were also introduced into a succession of experiments that were motivated by a search for new "conscious elements" other than "sensations" and "feelings." This merry hunt reached its climax at about the turn of the century with the work of Buhler, Watt, Ach, Titchener (194) and others. "Imageless thought" experiments followed in profusion.

As representative of the latter, Bagley's (5) study in the apperception of the spoken sentence is cited, which also reviews earlier work along these lines. In pursuance of similar aims, Jacobson (89) asked his subjects to fixate a word for one minute and to utter it with quick repetition in an effort to get at its "meaning," which in some cases was identified with "images" evoked by the experiment. In other cases "meaning" did not come to the subject despite a wealth of "images." All in all, "meaning" was found to be an evanescent "phenomenon."



Chapin (27) required students to introspect their "imagery" as she read Titchener's description of *his* (Titchener's) visual imagery of meaning as "the blue grey tip of a kind of scoop which has a bit of yellow above it . . . which is just digging into a dark mass of what appears to be plastic material." Results showed wholly visual imagery present in 50 per cent of the group, wholly kinesthetic in 36.7 per cent, and visuo-kinesthetic in 13.3 per cent.

Moore (128) investigated the temporal relations of "meaning" and "imagery" and found that "meaning" came before the corresponding name of an object. He also found that when his subjects were asked to react now to "imagery," now to "meaning," their reaction times were much longer in the latter case.

Weld (190) criticized Moore for failing to distinguish "meaning" from "process" and found various types of meaning in the reports of his subjects. McDonough (118, p. 426), in a study stemming directly from Weld's concluded:

The meaning is as truly a structure as the image, nor is the image the meaning. If that were so, the image would always constitute the meaning, a result which does not follow from laboratory experiments.

Sufficient account has been given of these early experiments to indicate that the general framework within which investigators worked, their purpose and their orientation were a reflection of the fashion of the times and that, as a result, such work places itself beyond the pale of language investigation defined in the widest possible sense, at the same time, they point to the incidental nature of the verbal stimuli employed.

#### *Statistical Studies in Language*

A group of studies which are, perhaps, farther removed from the language responses of individuals than the preceding are placed together because they share the same technique—namely, statistical analysis.

Using such a procedure, Zipf (201) has formulated a systematic approach to the observation, measurement and formulation into tentative laws of the underlying forces which "impel and direct" linguistic expression. For example, statistical study shows that the most common English word appears about once in every ten words; the next most common, once in every twenty words, et cetera, English being a mathematically harmonic language over nearly its whole extent. Similar tabulation shows that the greater the frequency of occurrence of a word in a language, the shorter the word. According to Zipf this great orderli-

ness is an expression of an underlying force which maintains an equilibrium between variety and frequency of word usage.

Thorndike (173, p. 405) has questioned Zipf's conclusions as evidenced in the following quotation:

I should think it would be in some measure a statistical artifact, adding things which lost much of their instructiveness by being combined. The relation between *a* and *b* presumably varies not only with the size of the sample, but also with the natures of the persons who speak and write, their knowledge of English, their purposes in speaking and writing, their past experiences their access to the speech and writing of others, and whatever else makes them and the language what they and it are.

Thorndike appears to object to the treatment of language responses that swamps out the manner in which an individual builds up and employs linguistic responses. Nevertheless, Zipf's work has stimulated other workers to similar research.

Using a statistical method and assuming that the relative strengths of associations in the vocabulary will be shown by the relative frequencies of associated responses, Skinner (162) analyzed the data reported by Kent and Rosanoff, and found that the mean frequency of the word most likely to be evoked by a given stimulus word was 258, that of the next most frequent, 121, third, 77, et cetera. The resulting curve for the 100 responses most likely to occur is approximately linear.

In another type of study, Skinner (163) tabulated the frequency of alliteration in 100 Shakespeare sonnets on the assumption that if their distribution were random it would argue against any "alliterative process." He found that, despite an apparent richness of alliteration, there was no significant evidence for such a process and concluded that

so far as this aspect of poetry is concerned Shakespeare might as well have drawn his words out of a hat. The thematic or semantic forces which are responsible for the emission of speech apparently function independently of this particular formal property (p. 191).

In a study related to the harmonic series law of word frequency distribution, Carroll (21) derived a curve to show the relation between the total number of words in a sample and the number of different words which may be used to indicate richness or variety of vocabulary. More recently Carroll (23) has developed a morpheme recognition test in order to determine the relationship between knowledge of English roots and affixes to vocabulary and Latin study and has found that the study of Latin influences morpheme knowledge but not vocabulary.

Richardine and Wilson (146) have made a statistical analysis of diaries kept by school children and report a total of 3,407 words, plus

280 numerals and 39 letter symbols. There were 603 different words used, 59 per cent of the words appearing only once.

Mosier's (130) psychometric study of meaning was conducted by presenting a list of 300 adjectives selected from Thorndike's word list to 150 college students for judgment as to degree of favorableness or unfavorableness expressed. Mosier concluded that meaning is composed of two constituents, one which is fixed for all subjects and a component which varies from individual to individual.

Yule (200) investigated sentence length as a statistical characteristic in writing prose applying it to two disputed cases of authorship. His results were completely consonant with the view that Thomas à Kempis was and John Charlier de Gerson (a one-time contender for the authorship) was not the author of the *Imitatio* (p. 377).

Williams (192) made the same sort of analysis of three samples of 600 sentences from the books of H. G. Wells, G. B. Shaw and G. K. Chesterton. The findings of both investigators show that stylistic traits in written verbal responses follow the same principle of individual differences applicable to all forms of behavior.

Boder (12) selected 388 specimens from American writings in the area of plays, law, fiction and science and found that a ratio designated as an adjective-verb quotient showed that the number of adjectives per 100 verbs varied with the different types of material, being highest for scientific work and decreasing respectively for legal and dramatic materials.

A final study by Johnson and Reynolds (95) made a factor analysis of 113 college students on 10 tests covering a wide range of verbal ability, which, according to the authors, shows evidence for the existence of two hypothetical "factors," F (flow of responses) and S (selection of responses).

### *Studies in Language Development*

*Vocalization.* There is a perennial interest in the manner of fixation of language responses out of the infant's early random vocalizations. Early accounts have been biographical as illustrated by the following quotation from Fenton (51):

There is . . . great richness and variety in the babble as a whole. Usually it contains a far wider range of separate sound than the child will have use for in later life. Among my baby's first sounds were a number of gutturals not used in English at all, among them the German, *ch*, and a harsh-throated *gh*. The German sound of *o*, and the French, *eu*, occurred frequently, and a considerable number of slurred vowel sounds. . . .

McCarthy (115) has presented evidence which supports Fenton's contention for the plasticity of the infants prelinguistic "mechanisms" in language functioning, and has also set forth the views of other investigators (116, pp. 278-315) on this question.

Experimental attacks on the problem have been made recently by Irwin and Curry (88) who transcribed the vowel elements appearing in the crying of infants under ten days of age and who tabulated those vowels that were used with greatest frequency. They conclude that the infant progresses toward an increasing use of back vowels. In another paper, Irwin and Chen (87) have reviewed the literature on the speech sound elements during the first year of life in a month-by-month analysis.

*Vocabulary and Parts of Speech.* Other aspects of investigation of the linguistic developments of children have centered about vocabulary and growth of parts of speech; consequently, "word count" studies have been on the increase. Thus, Jersild and Ritzman (92) found that an analysis of verbatim records of children's utterances during the school day showed that the amount of talking and the size of the vocabulary both increased with age. Brigrance (13) has reported that a comparison of parts of speech shows that as the percentage of nouns declines, the percentage of verbs increases, and has explained these results as indicating a growth of ability to express ideas in complete sentences. Further study of the developmental changes in the distribution of parts of speech by Davis (38) disclosed that the percentage distribution of parts of speech, over the age range from  $3\frac{1}{2}$  to  $9\frac{1}{2}$ , changed very little though use of nouns and regular verbs increased with age, while use of pronouns, conjunctions, and prepositions decreased.

In a related study, Carroll (22), calculated the number of determining and numerating adjectives occurring in 50 responses made by 87 children of 2.5, 3.5 and 4.5 years of age in both free and controlled situations and discovered more determiners, numeratives and corresponding pronouns with age increase but the proportion of such words to the total number of words remained fairly constant. However, the average number of words used by each child showed, as with previous investigators, a significant acceleration with age increase.

Davis (36) has similarly found that the number of one-word remarks decreased somewhat with age, that long sentences tend to be highly complex; and that a large percentage of adverbial clauses are used and a smaller percentage of noun clauses. The children of his study also were found to use long sentences when actually referring to situations confronting them or to topics closely related to the situation. Davis (35)

has also counted the number of proper names used by 436 children, aged 5.5, 6.5 and 9.5 in 21,800 remarks and found that the numerical importance of proper names was slight, constituting less than .5 per cent of all words used.

*Language and Sex Differences.* Certain investigators have attempted to find sex differences in amount, rate or patterning of linguistic responses of children. Representative of these studies is the work of Olson and Koetzle (135) in which combined data for two groups of nursery school and kindergarten children showed a verbal output of 16.5 words per minute (at a rate of 106 words per minute) while speaking under conditions of free play. Boys tend to speak less than girls during a given period of time but when speaking do so at a more rapid rate. Young (198) also confirms the verbal superiority of girls over boys.

*Language Development and Environmental Factors.* Pertinent here is the investigation of the language growth of children in two types of rural environments by Worbois (196), who found that subjects selected from a group attending a consolidated rural school showed superior verbal ability when matched with another group from a one-room school in the same area. Word recognition, word fluency and basic language skills test scores were higher in each case in the consolidated school group of subjects.

Onliness appears to be effective in inducing variety of word usage and increasing sentence length according to a study by Davis (37). Ninety-seven only children were found to be superior when compared with 166 twins and 173 non-only singletons. This linguistic superiority of only children was found to hold for both boys and girls.

Gansl (58) administered vocabulary lists to New York City public school children and found that in grades 8-14 boys were superior to girls and that there was a fairly constant increase in the vocabulary development of both sexes from the lower to the upper grades.

As a type of environmental control, Strayer (169) has observed the relative efficacy of early and deferred vocabulary training studied by the method of co-twin control. He concluded that

although Twin C had reached a higher level of accomplishment in the field of language at the end of 28 days of training than Twin T had attained after 35 days of training, when she was five weeks younger, Twin C had not then caught up to Twin T's accomplishment at the close of the experiment. Follow-up contact with the twins over a period of three months after the close of the experiment, however, showed that the differences were disappearing if not entirely gone (p. 316).

Further studies in this field have been reviewed by Anderson (3).



*Language and "Concepts"*

A whole block of experimental studies may be segregated from the preceding on the basis of their subordination of the linguistic response to some other behavior, the former serving as an index, or clue to a particular "concept," "class of concepts," et cetera. Stress is on the behavior "revealed" or "expressed" by the child's language.

A paradigmatic study in this field is that of Piaget (139) who made detailed analysis of children's conversations in order to trace out the growth of their thinking. Piaget's findings convince him that the thought of the child evolves from an egocentric and autistic type to the logical and socialized type of adulthood.

By means of a similar method, Kuo (110) has recorded spontaneous language from four Chinese children three to five years old, comparing these data with verbal filling-in-blank tests given to sixteen school-children seven to nine years old, and has reported that egocentric speech, which occupied from 10 to 20 per cent of children's conversation at the earlier stage decreased with age.

In a study of the use of pronouns by young children as a clue to self-awareness, Goodenough (62) has discovered that pronouns of the first person singular are used more often during play with other children than when the child is alone with an adult, while pronouns of the third person singular tend to drop out with age increase.

In similar vein, Young (199) has analyzed language records of 72 pre-school children for use of pronouns and, from the decrease of possessive self-pronouns and increase of possessive pronouns referring to others, concludes that there is a development of "social consciousness" with age.

A discordant note has been injected into the preceding harmonious theme by a recent study of Janus (90, p. 56) which indicated that

1. At all ages from 18 to 66 months, considerably more language is devoted by children to social manipulation than to egocentric remarks. . . .
2. For a variety of play situations, children exhibit much more the tendency to evoke a response in their hearers than to verbalize for its own sake.

Janus (90, p. 57) concludes that language responses show marked variability at all ages and that they are, to a greater degree, a function of the play situation rather than of age difference *per se*.

In regard to the function of "egocentric speech," Vygotsky and Luria (181) have demonstrated that egocentric or non-social speech is likely to appear when the child is confronted by a difficult situation as a help toward organizing subsequent activity. This function of

speech is never simply replaced by socialized speech but continues as a distinct process along with "external and internal speech."

Working from a different approach, Adams (1) found that among other things young children made more frequent reference to (had a "concept" of) the present and to past situations. Similarly, Davis (35) has reported that older children refer to places at a distance more frequently than younger children, who more often mention local places.

Shirley (160) has analyzed the speech of pre-school children for content, arguing that, since her subjects had not been exposed to the common environment of a nursery school, any community of response must derive from a common set of needs. These have been arranged in a hierarchy in the following order: (1) a need for succorance or security; (2) a need for independence; (3) a need for affiliation and another for recognition which tie for third place; and (4) those for nurturance, blame-avoidance and achievement which tie for fourth. Shirley's suggestion that further analysis of common responses of children made in free conversation might yield frequency tables for use in diagnosis of personality problems shows the "thing" orientation toward language that has been employed in parts of speech and concept studies.

Difficulties with the classification of the child's linguistic responses into the rigid pigeon-holing of the grammarian had been pointed out as early as 1894 when Lukens (114, p. 454) wrote "To classify such child-words by the adult distinctions of the parts of speech . . . is of course to be misled by very superficial considerations."

Apropos this general problem, in the same year Dewey (39, p. 64) said: "The psychological classification is to class the word according to what it means to a child, not to the adult with his grammatical forms all differentiated."

Shirley's (160) study referred to above also shows that "lookit," "see," and "watch what I do" may be used interchangeably to attract the attention of others. The same holds for the question, "What's that?" which is often rhetorical and is asked for the sake of gaining attention since the child often answers it himself immediately. These are some of the dangers of confusing psychological grammar with the grammarian's grammar.

A similar stricture applies to attempts at correlating "concepts" with words used by the child. In this regard, La Brant (85, p. 393) warns that "even so small a vocabulary as that possessed by the pre-school child offers difficulties. . . . The same word or spoken symbol may serve several purposes. *Orange* is both a fruit and a color; one word

to this child, two to that. *Good* has a certain meaning in *Be a good boy*, and a different one in *This apple is good.*"

### *The Clinical Psychology of Speech*

Because the literature in this field is so voluminous, embracing a wide variety of anatomical, physiological and behavioral pathologies; and because workers here are involved only in practical considerations aimed at relieving or curing those handicapped in transmissive or receptive speech, often without any interest or need in understanding the nature of language behavior, we need not further regard clinical studies in speech.

Parenthetically, it may be mentioned that investigations of stuttering have been reviewed by Travis (177), Solomon (164), Johnson et al, (96), Hahn (69) and more recently by Hill (78). Weisenburg and McBride (187) offer both a review of the literature and experimental investigations of aphasia, and Montagu (127) and Whitaker (191) have summarized work in the field of lip reading.

### *Experiments in "Phonetic Symbolism"*

Do the sounds of words naturally symbolize aspects of objects? Attempts to secure an answer to this question have been experimentally investigated beginning with Sapir's (151) study in 1929.

Sapir investigated the relation between sound and size and their corresponding word symbols by reading lists of nonsense words, differing only in one vowel, to a group of subjects who were to differentiate between the pairs on the basis of size. For example, "mal" and "mil" were arbitrarily given the meaning, "table," the subject being required to decide which of the two symbolized the large, and which, the small table. In general, Sapir found that the open vowels referred to the larger of two objects and closed vowels to the smaller in conformance with his belief that certain sounds have an "expressive symbolism" founded upon an "unconscious or intuitive logic which is not necessarily based on experience with the stimuli in their normal, functional aspect" (p. 239).

Newman (133) extended Sapir's study and included darkness and brightness, in addition to magnitude, reporting their differentiation through a phonetic symbolization. This was achieved by having the subjects select the larger of a pair of words differing only in their vowels (e. g., glupa-glopa).

Bentley and Varon (7) conducted a check experiment on Sapir's

and Newman's studies but with nine categories of object aspects; these included angularity, foolishness, endurance, liquidity, sentimental attachment, motion, noisiness, solidity and strength. They conclude that there is insufficient evidence for any *symbolic* values inhering in vowel or consonantal sounds as such and are of the opinion that Newman's comparisons were unwittingly made by his subjects on the direct basis of differences associated with the *sounds* themselves. Support for such a view is furnished by experimental evidence which showed that when subjects were asked to compare the magnitude of two simple sounds such as given by two tuning forks, the *lower* tone was reported "larger" than the higher in a ratio of about 7:1. With noises the *higher* noise was reported "smaller" than the *lower* in a ratio of approximately 20:1. As a final point, Bentley and Varon suggest that conceivable "magnitude" may be directly represented to the immediate visual perception of an articulating organism, the open mouth, the stretched jaw, the tensed lip, et cetera suggesting certain *associations* of magnitude. This factor may have operated in Newman's experiment but was ruled out in the Bentley and Varon investigation by screening the experimenter from the subject. These results suggest that here and there in a language certain words may have sounds with appropriate or possible *associative* values when the subject's attention is called to such associations, but to ascribe such symbolizations to the words themselves as reflections of "an unconscious or intuitive logic" is going beyond the facts, to say the least.

Another study in phonetic symbolism was performed by Fox (56) who presented a series of nonsense figures each accompanied by a list of six nonsense syllables, from which the subjects were required to select the most suitable name for the different figures. There was some agreement on the names chosen, especially for the sounds i, z, k, which were selected for sharp or angular figures and m, l, u and b for smooth or voluminous ones. The authors do not appear to relate these results to associative factors but do offer such a general explanation as indicated in the following statement quoted from their study: "With general instructions, O's tend to report as reasons for their choices associations involving their knowledge of languages and of familiar physical objects (56, p. 578)."

Panconelli's (138) results are more in line with those of the Bentley and Varon study. This author subjected natives of Hamburg to the noises of sawing and pounding. The subjects reproduced the stimuli with widely varying symbols that corresponded more to the "hearer's ideational world" than to the actual auditory stimulus. Consequently,

Panconelli believes that he has refuted Sullivan's theory which claims a close relationship between the sounds of natural phenomena (such as thunder) and their phonetic symbolization.

It may well be that the reason why these findings contradict those of Sapir and Newman lies in the methods employed since Panconelli's subjects had no associative aids from nonsense syllables that inevitably bear some resemblance to actual words. Another criticism against claims of the appropriateness of verbal symbols to the object symbolized has been raised by Kantor (101, p. 171) who points out the prejudiced selection of cases that completely ignores exceptions to the alleged rule. A final point pertaining to such studies in phonetic symbolism may be put as follows: How are such experiments related to the language actions of organisms? Do they throw light on such behavior?

#### *Experiments in Language Employing the Association Technique*

Repeating an earlier experiment of Thumb and Marbe, Esper (46) used association reaction time and found that the more common a given response, the shorter its reaction time and that verbal stimuli tend to be associated with word responses belonging to the same grammatical category (e.g., adjectives are associated with adjectives, numerals with numerals, et cetera).

Another experiment by Esper (47) was designed to study associative interferences and the development of linguistic categories in artificial language materials. Four shapes and four colors were used in 16 different combinations, each of which had a nonsense name assigned to it consisting of two syllables correlated throughout with the two factors of shape and color. The order of the linguistic "elements" was color-shape, conforming to the English grammatical construction, adjective-noun (e.g., blue triangle). Results showed that the names of figures confused with one another during learning tended to interfere with each other in articulation. Greatest associative interference obtained in the case of linguistic material deviating from customary modes of syllable division. It was also found that names of figures of a given category tended to be confused and assimilated to the name of a particular figure of that category.

Adopting Esper's technique, Wolffe (193) analyzed the errors made in naming nonsense figures, such names consisting of size-name and form-name elements, the size-name syllable coming first. Analogic changes made both during learning and in the retention tests were of two kinds: those in which a subject viewing figure A would give it the name belonging to B, and those in which he would respond with a



mixture of names belonging to A and B. Associations followed the categories of size more than of form, and 55 per cent of all errors were due to interference on the part of some other figure of the same shape as the one on which the error was made.

Since it was not clear if the first syllable in the dissyllabic names used was more unstable because of its position or if such instability was inherent in size names, Wolffe (193) repeated the above experiment reversing the order of the syllables and concluded that "the position of a syllable is of much greater importance than its meaning in determining its stability or instability" (p. 315).

A subsequent experiment by Esper (48) showed that instability of verbal responses occurred when stimulus objects, because of common features, set up an associative interference so that the sound or articulation corresponding to one evokes the symbol for the other one, such associative interference being indicated by "simultaneous or successive activation by one stimulus object of two or more response mechanisms, and where the tendency is strong, by lengthened reaction time in the verbal response to the object (p. 378)."

Skinner and others have perfected several ingenious devices for studying phrase or sentence associations rather than words as in Esper's or Wolffe's studies. Skinner's (161) verbal summator is a phonograph record which presents faint and indifferent sound sequences to the subject who is urged to hear them as words, phrases or sentences. Continued reaction usually elicits some associated verbal response from the subject's repertoire of verbal behavior.

Estes (50) has developed a visual form of the verbal summator which exposes skeletonized words, phrases or sentences presented tachistoscopically for 1/10 second.

The fact that the subject is urged to report the "first thing that flashes through his mind" places these experiments in the field of suggestion although Skinner and Estes explain the subject's behavior in terms of a response to the skeletal material which somehow "summates" with a strong "latent response," the response serving as an index of the "strength" of such latent response. The associative value of Skinner's technique has recently been adapted to the diagnosis of abnormal behavior by Trussell (178) after the manner of the Rorschach test.

"An experimental study of the effect of language on the reproduction of visually perceived form" by Carmichael, Hogan and Walter (20) bears certain resemblances to the techniques utilized by Skinner and his colleagues, in that suggestion and association is involved in both. In this experiment a set of 12 relatively ambiguous figures was pre-

sented to subjects. However, one list of names was given to the figures just before presentation to one group while another list was applied to the same figures prior to exposure to another group. Later reproduction of the figures by the subjects was generally in the direction of the names applied to them. According to these authors, the results "may at least tentatively be described in terms of a dynamically considered process of association" (p. 86).

Starting with the assumption that language is a form of conditioned behavior, and reinforced by the phenomenon of generalization gleaned from experiments dealing with verbal and other conditioning stimuli, Cofer and Foley (29) recently lay the theoretical groundwork for a series of experiments involving the word association technique.

As a follow-up study, (53) they attempted to determine whether or not generalization in word associations could occur along more than one homophone gradient from a given word and along synonym gradients and synonym gradients two transformations removed from the original word. Their findings showed generalization along the predicted dimensions in support of their theoretical analysis.

Subsequent work by Cofer, Janis and Rowell (30) demonstrated generalization along antonym gradients, while Foley and Mathews (55) showed a greater degree in mediated generalization via Spanish words, such "gradients" having been established for their subjects during a regular course of instruction in Spanish. The Control group with significantly lower recall scores received no such "reinforcement."

Recently Foley and MacMillan (54) showed differences in the type of verbal responses given by members of different professional groups "thus affirming the occupational conditioning of verbal reactions and suggesting the possibility of operation of mediated generalization along 'professional gradients' (p. 309.)"

Belonging to this general area of investigations dealing with verbal materials, is an experiment in "Some Complications of Associative Processes" by Thorndike (175). Thorndike presented a list of 112 paired associates, many members of the first pair of which were rhymed words. His findings in the recall test showed that a mental set had been created which tended to elicit many unheard of, and meaningless words (this set apparently created by the meaninglessness of the connection between the paired associates). Many words similar in meaning to the *second* members of the pairs and many rhyme responses were also the result of "mental set" created by conditions present during the reading of word-stimulus pairs.

That words may function as substitute stimuli the same as other

objects is apparent from another study of Thorndike's (172) in which subjects were asked to rate words for pleasantness or unpleasantness of the sound alone without regard to their meaning, such responses showing influence of past associations. This was confirmed by further use of nonsense words which were rated pleasant or unpleasant on the basis of their *resemblance* to meaningful words that had one or the other type of associative value. The differences commonly assumed for the superiority of vowels and liquids and the inferiority of gutturals and aspirates were disproved by artificial words composed of those sounds. The controlling factor was association.

### *Gesture*

Gestures as a species of human behavior have been sadly neglected. The greatest of psychologists, Wundt and Freud, have not exactly neglected them, but have done little more than mention them. Until recently no one tried to study them experimentally, and even now there are more problems connected with them than any psychologist could hope to solve in a life-time.

—M. H. KROUT (109)

One need not agree with Krout *in toto* to acknowledge the indifference of psychologists to the functioning of gestures in linguistic responses. While Kantor (101, p. 160) has indicated the equivalence of gestural to buccal action in referor language behavior and has criticized linguists for failing to incorporate them into language study, yet observational or experimental studies of gestural language are practically non-existent.

Krout (108), for one, has made an analysis of the social and psychological significance of gestures differentiating acultural gestures such as pre-natal, congenital, animal and infantile movements, from conventional, pseudo-conventional and autistic gestures. He (107) has also experimentally investigated autistic gestures, developing a method for observing them and testing the consistency with which they reappear. At the same time, Krout has demonstrated that autistic gestures originate in conflict situations.

Critchley (32) has done no experimental work but has called attention to gestural modes of communication which may be used independently of, or in connection with, oral speech. He is of the opinion that besides symbolic gestures of a conventional sort that have been culturally established, there are also instinctive gestures comprehensible to all in utter disregard of international boundaries.

Paget (136) has estimated that it is possible to make 700,000 differentiable gestures by combining postures and movements of upper arm,

forearm, wrist and fingers, which would make the hands 20,000 times more versatile than the mouth. In view of the possibilities, Paget has experimented with the development of an easily learned universal gesture language that is based upon Ogden's Basic English.

Efron (42) has made a pioneer study of the frequency and kinds of gestures among Jews and southern Italians. He has compared members of these two groups who were each more or less culturally isolated with those who have been assimilated, and has pointed out differences in the style and frequency of gesturing and its elimination in the assimilated members of both groups. Since Efron's work is anthropological in scope, there is little concern with the psychological nature of gesture; rather, his interest is in the differences of gestural patterning in different groups.

All in all, investigations of the gestural aspects of language responses have hardly had a beginning; nevertheless, it would seem that this field has possibilities of development once proper techniques have been worked out.

#### *Studies in the Perception of Language*

The factors pertaining to the perception of linguistic stimuli have been investigated by several students with rather interesting results and implications. For example, Howells (80) and Howells and School- and (81) have worked out a test consisting of standard speech stimuli recorded on phonograph records along with other conflicting sounds, played loudly to 100 subjects, the number of words perceived by each subject constituting his score. Speech perception scores showed negligible correlation with two reliable tests of auditory acuity, indicating that the acoustic factor in the perception of speech has been greatly overestimated. These investigators conclude that: "The common assumption that ability to hear speech is analyzable into elementary factors, or acuity thresholds, is unwarranted on the basis of available evidence (81, p. 345)."

Preston (143) analyzed the speed of word perception and its relation to reading ability, and discovered a significant difference between the perception times for familiar and unfamiliar words. He also found that perception speeds showed a relation to the subject's speed of reading and to his vocabulary knowledge.

A final experiment by Cotton (31) concludes the type of investigation carried on under this category. Assuming a preponderant factor of lip reading in the ordinary perception of speech by normal individuals. Cotton used a small, sound-proofed room with double glass windows

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and made quantitative measurement of this visuo-perceptual component of speech reception. The speaker inside the room could be made visible by varying the inside illumination; at the same time, his speech could be distorted. In general, the results showed that when speech distortion occurred while the speaker was not visible, his speech was practically unintelligible to the subjects; but proper comprehension resulted from the same amount of distortion when the speaker was simply made visible to the audience. While not, by any means, complete this brief survey of perceptual problems investigated shows some of the factors related to language behavior and serves to place the latter in its proper perspective.

#### *A Study in Language Patterning*

Lately Thorndike (174) has investigated some of the functional features of language in an attempt to find the changes in meaning that a word undergoes when it is changed grammatically as from adjective to verb (e.g. to beautiful). He found the comprehension of such radical modification of speech patterning to be heavily dependent upon the immediate situation. In applying his findings to historical changes of words toward abbreviated forms, Thorndike believes that economy of effort rather than desire for variety motivate the truncation of words with frequent use, and thinks that desire for word variety is a cultivated want. In interpreting the avoidance of too frequent repetition of words by literary people, Thorndike suggests that "the explanation probably lies in a rather fundamental tendency of the neurones to be repugnant to action after a certain number of actions within a given time (p. 37)."

#### *A Study in Language Behavior*

Starting from a somewhat different orientation, Bucklew (17) has recently designed a number of experiments to study speech reception. The first experiment was meant to determine the speed with which language serves to refer individuals to the things spoken about. A series of 10 slides was presented, (a) simultaneous with the beginning of speech to one group of subjects, and (b) at the end of speech for the other group. The speech, presented phonographically, referred to various aspects of the illuminated slides confronting the subject. Time of response was measured by means of a chronograph from the beginning of speech until the subject pressed a true or false key indicating the truth or falsity of the statements pertaining to the slides. Results showed an average increase of .8 second from concurrent to serial presentation; consequently, Bucklew concluded that language func-



tioned more efficiently when the slide was presented at the beginning of speech and related these results to an Interbehavioral theory of language.

A second experiment attempted to find whether speech distortion disrupted a language response more when the stimulus object referred to was present or absent. Ten sentences, a normal and a distorted version of each, were phonographically presented to 66 subjects. When the stimulus object referred to was absent, the percentage of errors increased to three and one half times that for the concrete situation (stimulus object present). Introspective reports showed that about 4 out of every 5 subjects reported the distortion more disturbing in the former situation.

A check was made upon Experiment II by distorting the *same* words in both concrete and remote situations in an identical fashion; it was found that there was no significant rise in RT in the concrete situation but an average increase of .36 seconds was found for the remote situation, while per cent errors rose to 13 per cent. Introspective reports also showed that about twice as many subjects judged the remote distortion more disturbing than the concrete. Bucklew concluded that speech distortion is less likely to interfere in speech reception where the referent is before the subject.\*

### III. THEORY IN PSYCHOLINGUISTICS

#### *The Need for Language Construction*

The preceding account of studies in language proves that there has been no dearth of research in this area. Apparently too, students of language have been about as ingenious in devising apparatus and techniques and in designing experiments as investigators in other fields. Yet, despite a wealth of interest and of work, there seems to be no corresponding increase in our understanding of linguistic responses. Descriptions of such common, ordinary every-day behavior as occurs in the case of two human organisms conversing (behavior, incidentally, which infiltrates all other forms of behavior) is conspicuously absent in our most up-to-date text books in psychology (41, 91, 124, 194) or where developed in scanty fashion (129, 131, 148), they follow traditional prescriptions known equally well to the man in the street through his cultural heritage.

\* This survey of behavioral investigations in language is, by no means, exhaustive. The attempt has been to give a sketch of the range of problems included and modes of attack employed. However, further references are available in the work of Esper (46, 49) Adams and Powers (2), Kantor (99-104), and McGranahan (119) and Metcalf (125).

Such a state of affairs may well be the reflection of a lack of proper orientation toward linguistic behavior. It is quite likely that what theory† has been developed has been found disserviceable in unifying linguistic research and in pointing the way to further, fruitful investigation. And, again, where psychologists have either ignored or shunned theory, or failed to make explicit their assumptions about language, they have been prevented from attaining a first, crude approximation at the isolation of language responses and the segregation of them from other classes of behavior. As an example of such uncritical procedure, there is the case of a recent writer (144, Chap. 6) whose implicit (and unwitting?) attachment to an "Expressionistic" theory of language has caused him to include such disparate behaviors as block-building, drawing, and mathematics within the language category.

Further testimony of the psychologist's incompetence in the language area has come from many quarters. Speaking with reference to the need for a psychology of language that might have usefulness in language teaching, Buchanan and MacPhee (16, p. 25) write as follows: "The present interest of language teachers in psychology . . . is frequently a source of embarrassment to the conscientious psychologist."

Huse (85) has also presented a destructive criticism from language teachers concerning (a) the best methods and materials for language learning to be deduced from the contradictory findings of experimentalists and (b) the apparent lack of understanding of the psychological processes involved in language.

Woolbert (195) and Eisenson (43) have both expressed a like need in the teaching of public speaking. This awareness of the indispensableness of some sort of psychological basis for their own procedure on the part of non-psychological students is not difficult to understand when we note that all investigators draw upon the same event, namely, humans talking to some one about something (i.e., responding to stimuli). Their individual abstractions require a psychological description at many points for filling in the completed picture. Thus, Bloomfield (11, p. 78) has surrendered the problem of "meaning" entirely to the psychologist while Jespersen (94, p. 17-18) is found involved in the considerations of such activities as speaking and hearing and other behavioral items. Another case in point is Hagboldt's (68, pp. 84-91) reflections on the psychological aspects of reading. Palmer (137) and Gray (63) have expressed similar dependence on psychology and, among

† The term, theory, is not used in the present paper in the sense of a statement in terms of some hypothetical correlate of language action, but refers to an observationally-derived description of what happens when organisms behave linguistically.

among psychologists, Esper (49) has shown a sensitivity to the need for language construction.

Although a need for linguistic orientation has been expressed on all sides, language construction has been of an inadequate sort at best, so that as a way out of this predicament, people in related fields of language study have done one of two things. They have, in some cases, taken whatever feeble theoretical framework has been available or else they have gone ahead in constructing their own behavior foundations without the psychologist's help. In either case, insofar as such workers involve themselves in behavioral consideration, they become legitimate matter for our observation. It would seem worthwhile, then, to evaluate the present status of language construction from the psychological viewpoint. To this end, we turn to a review of sample definitions of language wherever they may be found, without distinguishing between the technical psychologist's construction from that of the worker in related fields. Whatever enters the field of language construction in its behavioral aspects is a legitimate object for our examination and dissection.

#### *Dualistic Language Theory*

Mentalistic language theory may take one of two forms. Where stress is on the "psychic" *deus ex machina* that converts things into thoughts, thoughts into word images, et cetera, we have the typical expressionistic design. Where prominence is given words as symbols mediating between a mental state and a thing, it is indicative of symbolic construction.

#### *Expressionistic Construction*

*Wundt.* Language as the instrument for the expression of thought was first systematically developed by Wundt (197, pp. 60-61) who traces its origin to gestures involuntarily expressing strong feelings. These arouse corresponding feelings in others. The displacement of emotional by ideational "psychic" components, which become voluntary, leads to an exchange of thought; this is language. Darwinian evolutionary influences are everywhere apparent in Wundt's attempts at language construction.

*Bloomfield (1914).* At this stage of his development, Bloomfield (8, p. vi) builds squarely upon Wundt's foundation. The following quotation clearly shows the Wundtian stamp and places him at once within the "Expressionistic" fold. In addition, it demonstrates Bloomfield's investigative procedure from such a psychological base.

True to its original form of an outcry under the most violent ex-

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periences, language is most realized under emotional stress. Some violence of experience must normally be present to call forth loud expression. If this emotional violence is the dominant cause of the utterance, we speak of exclamation (p. 70). With communicative motive, this is weakened to *declarative* utterance, and further to a *question*, which is more of a "transference to communicative use of the exclamation" (pp. 70-71).

*Stinchfield.* Stinchfield (168) assumes a "language" without any specification of the matter under consideration, but describes the machinery for an expressionistic theory when she details the transformational processes by which "sensations" "arouse" "images,"—clusters of the latter composing "ideas." "Words or symbols which stand for ideas are stored in the sensory areas for vision and audition, and in the motor speech area, whence the impulse goes down over the motor pathway (p. 90)."

*Pillsbury and Meader.* One of the rare textbooks which attempts technical analysis of language from the psychological viewpoint is by Pillsbury and Meader (141). This work follows the orthodox Expression doctrine when it construes language as "a means or instrument for the communication of thought, including ideas and emotions" (p. 4). The exact details by which ideas are converted into language are well worth quoting.

... man thinks first and then expresses his thought in words by some sort of translation. To understand this it is necessary to know how the words present themselves in the consciousness of the individual, how they are related to ideas of another type than the verbal, how the ideas originate and how they arouse the words as images, how the movements of speech are evoked by these ideas, and finally how the listener or reader translates the words that he hears or the word that he sees into thoughts of his own.

Speech has its origin in the mind of the speaker or writer and the process of communication is completed only when the word uttered or spoken arouses an idea in the listener or reader (pp. 92-93).

Pillsbury and Meader (141, p. 16, Fig. 1) offer a diagrammatic representation of the elaborate speech processes as they have described them above. The train of events begins in the speaker or writer with thoughts or feelings and concomitant mental imagery, which are shown to innervate the muscles of the lips, tongue (or hands in the case of writing). The result is either ether vibrations or air vibrations carried to the eye or ear (respectively) and to the hearer's brain where they are *transformed* into "sensations." These last, then, give rise to an "idea" corresponding to the "idea" in the speaker that started the whole series of mystical operations.

*Gardiner.* Gardiner (59) has criticized Expressionistic theories of language because "expression of thought" is such a minor function of language, more common ones being "to inform somebody about something, to exhort somebody to do something, or to win sympathy from somebody in respect of something" (p. 64).

So conceived, speech is said to comprise four factors: the speaker, the listener, the thing spoken of, *and* the actual words. Since Gardiner has reified the speaker's action into "word-signs" (p. 76) or "clues" (p. 80) his theory may therefore also be classed under symbolic types of construction but more detailed specification shows that in a typical situation (pp. 62-80) the speaker, *having* a "thought" of some *thing* (this transformation being essential), gets a corresponding "sound image" of a particular word, then volitions to articulate it, thus "transmuting the psychic word" (Fig. 4, p. 91) into a physical disturbance which operates in like fashion for the hearer. Essentially the machinery constructed here easily fits the Expressionistic mold, and although a dual classification is possible, Gardiner is placed in this category as a matter of convenience.

*Gray.* Since constructional details of the Expression doctrine show little variation from author to author, it will be sufficient merely to quote Gray's (63) general approach to language which is conceived as "a physical and external manifestation of a non-physical (emotional, intellectual, spiritual) and internal state, an endeavor to represent materially what is essentially immaterial (p. 15)."\*

Lest the reader think that such construction belongs to the hoary past, we again call attention to the aforementioned 1941 publication (144) to show the insidiousness with which this theory has crept into current psychological thinking. It will be recalled how block-building, drawing and mathematics were identified with language much after the manner of Croce's (33) extension of Wundt's Expression idea to art in the former's identification of the linguistic with the esthetic.

The penetrating influence of Expression doctrine is grossly apparent in the most recent text books, as the following examples from Child Psychology demonstrate. For one, Hurlock, 1942 (84, p. 157) uses the term, language, to refer to "every means of communication in which thought and feelings are symbolized so as to convey meaning, involving such widely differing forms of communication as written, spoken, sign, facial gesture, pantomime, and art (sic)." Speech is only one sub-type of language that requires articulate sounds or words to convey thoughts.

\* Attention is directed to the date of publication of Gray's *Foundation of language*, which is A.D., 1939.



Nagge (132, p. 274) is also seen to cling to such out-moded theory when he states that "vocal language is the means whereby words become a method of expression of ideas and feelings." Further evidence of Expressionism is to be noted in his "animal grunt" theory of the origin of language, deriving from Wundt in his definition of a sentence which is said to be "a chain or series of words expressing a complete thought" (p. 285).

Expression theory is, therefore, by no means dead. Viewpoints in psychology as in other sciences, once established, die hard. One perpetuating factor is a pretended or avowed disinterest in language theory on the part of text book writers, an indifference which militates against a sensitivity to currents of thinking in the cultural atmosphere that consequently insinuate themselves into psychological writings.

### *Symbolic Theory*

*Jespersen.* With keen discernment, Jespersen (93, p. 7) has realized that language has no disembodied existence—that it is not an entity in the sense of a beach or a dog but must be conceived as a function of human beings—and although he considers communications in the light of "habits," it is a dualistic psychology that forces him to treat language actions as "things," e.g.,

A word . . . may be rightly compared with such an habitual act as taking off one's hat or raising one's fingers to one's cap: in both cases we have a certain set of muscular activities which, when seen or heard by somebody else shows him what is passing in the mind of the original agent (sic!) or what he desires to bring to the consciousness of the other man (or men) (p. 8).

Language is, therefore, no longer "action" but is reified as a series of things or symbols standing for, or evoking thought. Incidentally, one may see Expression doctrine rearing its ugly head in the near background.

*Ogden and Richards.* Besides having emotive and other functions, according to Ogden and Richards (134), language also performs the symbolic function of standing for things. But the relationship between symbol and referent is not direct; for there is an intervening factor, a "thought," which mediates between the two. Grammatical study imputes a direct connection between symbol and referent but the connection is really an indirect one around the two sides of a triangle (134, p. 11) with "thought" at the apex, and "symbol" and "referent" at each side of the base (the base line being shown dotted to illustrate the indirect relationship). In addition to objectionable, psychic implications inherent in such theory, there exists the confusion of genuine sym-

bolic behavior with linguistic responses as indicated by Kantor (102, pp. 66, 67, 103, 104).

*Sapir.* For Sapir (152) it is more satisfying to study the more dependable outward garments of language but its psychic correlate must be taken into account at the very outset as noted in his definition of language, which is a

... purely human and non-instinctive method of communicating ideas, emotions, and desires by means of a system of voluntarily produced symbols (p. 7).

*How these symbols work—*

the speech element "house" is the symbol, first and foremost, not of a single perception, nor even of the notion of a particular object, but of a "concept," in other words, of a convenient capsule of thought that embraces thousands of distinct experiences and that is ready to take in thousands more (p. 12).

*How "concepts" sometimes evaporate—*

Each element in the sentence defines a separate concept or conceptual relation or both combined, but the sentence as a whole has no conceptual significance whatever (p. 13).

*Pieron.* There is no organized presentation of language as symbolism in a book treating with other problems but because of the intimate connection between language and thought demanded by a symbol theory that has been incorporated within the work, their kinship comes out here and there. On p. 30, for example, Pieron (140) refers to language "as the principal form of symbolization" and on p. 48 it is "the essential instrument of 'symbolic thought'." This, at least, sketches the outline of a theory developed more fully elsewhere by this and other authors.

*Buhler.* For Buhler (18) language has three functions,—Expression, Evocation, and Representation. The *expression* function is analyzed out from a consideration of the conversion of objects into thoughts and thoughts into articulations from the viewpoint of the speaker. From the standpoint of the hearer, speech *Evokes* thoughts or ideas of the things which it symbolizes. While these two functions of language are important, its nature has not been described until one has given an account of *Representation*, which is the relation between the speaker's action here reified into conventionalized symbols and the things referred to. The two fields which are important in Buhler's elaborate development of the Representational function of language are the Demonstration Field (Zeigfeld), in which the Expressor and the Evoker of language are oriented and the Symbol Field (Symbolfeld) constituting the linguistic factors in which the symbols are imbedded. The latter includes the more formal features of grammar such as case and tense and the associated

factors or "spheres" which certain words carry as when "accident" suggests "automobile."

Palmer (137). Language defined:

Language has properly three functions: it expresses the thoughts, feelings, etc. of the speaker; it influences the behavior (in its widest sense) of the hearer (the evocative or affective function); and it symbolizes the "things" to which it refers. Since the first two functions cannot be carried out without the last, this means that symbolization, the peculiar alignment of sound and meaning, is the constitutive factor of language. Language may then be defined as the symbolic system in which vocal sounds constitute the material of the symbols (p. 1).

The resemblance to Buhler's theory is quite obvious.

Eisenson. In this recent (1940) text book on *The psychology of speech*, Eisenson (43, p. 3) asserts that "language is any system of recognized symbols used to produce or prevent specific responses of thought, or feelings, or actions." A symbol is further defined as standing for something else. "It implies that a connection of some sort has been made between a word and an object, or a word and an idea. . . . The word is the sign for the object or idea (p. 4)."

#### *Summary of Symbol Expression Theories*

A review of the mentalistic procedure along constructional lines shows that a characteristic feature of their work is the bifurcation of a speech act into two discrete components—an inner psychic "thought" or "idea" and an outward material correlate, the "word" or "symbol." Fundamentally there is little difference between them. Such differences as exist involve only points of emphasis and elaboration of an analysis which has succeeded in resolving language behavior into *form* and *content*. Where emphasis is on the form side which considers how thoughts get transmuted into words and words into thoughts, we have Expression theory such as Pillsbury and Meader, Gardiner, and Gray previously referred to have developed. Where Expression theory generates insecurity in dealing with the "inner phase" of language because "its inner meaning, its psychic value or intensity, varies freely with attention or the selective interest of the mind, also, needless to say, with the mind's general development (152, p. 13)." then one stresses words, "the outer garment of thought" (p. 14). In either case, one always has one or the other partner of the psychic bi-polar "thought-symbol." One escape from this dilemma is behavioristic construction to which attention is next directed.

*Behavioristic Attempts at Language Construction*

**Watson.** Behavioristic language construction has shown distinct progress in the direction of objectivity by extruding all psychics from the description of language action. As early as 1914 Watson (183, p. 328) insisted on placing language habits on a par with other bodily habits. In *Psychology from the Standpoint of a Behaviorist* (184, Chap. IX) language habits were anchored down to "the whole body but specifically to the neuro-muscular system in the head, neck and chest segments" (p. 310). There is little technical improvement here, however, since Watson's chief purpose was to connect implicit language habits (thinking) with explicit speech habits; but, by 1930, in his book, *Behaviorism*, (185, pp. 228 ff.) language habits are fitted into the conditioned response framework adopted from Pavlov. Except for this rigid machinery, Watson may have come close to an adjustmental view of language action. Under the circumstances, words are treated as substitutes for objects that become organized into a series of conditioned responses. Muscular stimuli come to serve in place of visual stimuli as illustrated in the following quotation:

Suppose you read from your little book (your mother usually sets an auditory pattern), "Now—I—lay—me—down—to—sleep." The sight of "now" brings the saying of "now" (response 1) the sight of "I," response of saying "I" (response 2) and so on throughout the series. Soon the mere saying of "now" becomes the motor (kinaesthetic) stimulus for saying "I" (185, p. 235).

It is to be assumed that the hearer responds similarly through a series of responses in the same manner as the speaker advanced beyond the stage illustrated above.\* With this approach, language is reduced to a mechanical series of conditioned responses; although it is questionable if such a formulation does full justice to the language action of an individual engaged in conversation. We are in sympathy with Seth and Guthrie (159, p. 76) who ask *a propos* this general problem:

How is it then that the right words occur? How is it that speech keeps to the rails of common sense and intelligibility? Mere association . . . is as likely as not to take us off at a tangent into incomprehensibility. If I start out to say, *The pack of cards is on the table*, the mere activity of association is as likely to carry me from the word *pack*, to *hounds*, or *wolves* as to *cards*.†

\* Watson's (186, Vol. 3, p. 329) recent statements on language are substantially unchanged.

† These authors show insight into the importance of the general speech situation in guiding the speech pattern of the speaker when they mention his "attention upon a particular object, the 'thing meant'" (159, p. 76).

Is not our everyday conversation more like the adventure of faith shown by the British orator, Charles Fox, who got himself into the middle of a sentence and trusted Providence to get him out again?

A chief complaint against Watson's theory is that it leaves more unsaid than explained. It circumvents problems by refusing to face them squarely, avoiding them merely by scantiness of statement. Robinson's (147, p. 86) criticism is quite pertinent because it points out how inadequate Watson's mechanistic-limited system is in dealing with behavior events; therefore, we quote him fully.

On the ordinary view, which admits the appeal to the consciousness of the agent,\* the child has learnt that the word "'means' the 'thing' . . ." But the Behaviorist has no business to know that the child "desires" or "wants" the box. The consequence is that if the Behaviorist is to be faithful to his initial abstractions and keep within the limits he has laid down, he must define his substitute response simply by the place it occupies in the general "pattern" formed by all responses, implicit and explicit. But what is there in the nature of a laryngeal response as such which can possibly label it as a substitute for the response of "the larger musculature"? The only differentiating circumstances possible are order in time and frequency of accompaniment and their insufficiency is too patent to need comment. Consequently, the notion of substitution does not seem to work; it is by no means clear what "relevancy" could mean in the theory under discussion.

In the same symposium dealing with the identity of thinking and language, the substitute nature of Watson's "response" is again discussed by Thomson (171, p. 65) who interprets Watson to mean by substitution a "relevant substitution." Quoting Watson's account of a child learning to say "box" instead of reaching for box, Thomson asserts:

"Box" is not true language when it merely accompanies the thing, but only when "the child learns that the uttered word is a sufficient stimulus to cause the attendant to hand him the objects he names without actually having to execute bodily movements with respect to them" (p. 66).

A final comment by Bartlett and Smith (6, p. 56) sums up the discussion on the inadequacy of Watson's concept of substitution. That Watson implies something more than his postulatory set-up permits is apparent in the suggestion made by these writers that a conditioned reflex level of functioning might establish a vocal habit at best but could not contain, in addition, "later associative connection of the word when learned with the bodily habits connected with the object for which the word stands" Watson, (184, p. 320).

\* Although the mentalistic system espoused by Robinson is no more satisfactory inasmuch as it treats language behavior in antinaturalistic terms, his ability to handle the speech event within his postulatory system must be admitted.



In the writer's opinion, Watson would like to eat his cake and have it, too. When he finds that his system is inadequate for handling language responses as conditioned reflexes, he goes beyond the confines defined by his postulates and squeezes in an explanatory statement legitimately belonging to viewpoints regulated by radically different postulates. While not squarely faced and developed, his acknowledged realization of the "relevant" substitute-stimulus function of words, permits classifying his rather dilute language construction within the symbolic category.

A final point is to indicate the substitute stimulus idea germinant in Watson's construction but waiting for full development by future "Behaviorists." That he did not advance his construction more elaborately may be appreciated from his serious attempt to objectify thinking by identifying it with language.

*Weiss.* Weiss introduces a variant feature into behavioristic language theory with his stress on the social function of speech and subordination of the anatomical involvements of language responses. The dual character of the linguistic response functioning either as a response to a stimulus or as a stimulus for a response is said to be an advantage for social cooperation and communication. Its substitute stimulus function is developed in *Linguistics and Psychology* (188, pp. 53 ff.) as well as in his later text. In the latter (189, p. 313) a word "becomes a bio-social stimulus (symbol or signal) for sensori-motor interchanges between individuals."\* Again, "the language response as a substitute stimulus for actual objects or situations allows the bio-social reproduction of practically any stimulating conditions that have ever occurred at any time or place" (p. 324). In short, if one overlooks the social emphasis that Weiss gives to language, his formulation is not essentially different from that of Watson; and, while the following statement is taken from Weiss (189, p. 288) it may characterize both, for it demonstrates the mechanical and serial action which both ascribed to language: "When I respond to the voice of my fellow man," Weiss writes, "I react to the series of sounds that he produces."

*Bloomfield—since 1925.* Feigning an indifference to psychology, Bloomfield (11, p. 153) throws overboard the mentalistic system of Wundt which did service for him in his 1914 text (reviewed above) and proceeds alone in his analysis, although here and there in his writings one may see traces of Weiss (c.f., e.g., his *Why a Linguistic Society?*) (9).

In his *A Set of Postulates for the Science of Language*, Bloomfield (10, pp. 154-157) starts from a foundation in which the existence and

\* Mead's (123) influence is here apparent.

interaction of social groups held together by language is granted by psychology. Furthermore psychology gives this series:

to certain stimuli (A) a person reacts by speaking; his speech (B) in turn stimulates his hearers to certain reactions (C). By a social habit which every person acquires from his elders, A-B-C are closely correlated. Within this correlation, the stimuli (A) which cause an act of speech and the reactions (C) which result from it, are very closely linked, because every person acts indifferently as speaker or hearer. We are free, therefore, without further discussion to speak of *vocal features* or sound (B) and of *stimulus-reaction features* (A-C) of speech.

From this point on, having once split the speech event, Bloomfield is confronted with *forms* (vocal features) on the one hand and *meanings*, (the correlated stimulus-reaction features of utterances) on the other hand. Now, since meanings (thus postulated) cannot be further dealt with, least of all analyzed into smaller units, then "This is no doubt why linguists, confronted with the parallelism of form and meaning, chose form as the basis of classification (10, p. 157)."

But leaving for the moment the problem of meaning, which bedevils Bloomfield continuously, we turn to his more general treatment (11) of language utterances, especially as it involves itself in psychological analysis. In principle, there is here nothing other than in preceding behavioristic systems. Note, for example, that "The language of any speech-community appears to an observer as a complicated signaling system . . . (p. 281)." "The phonemes . . . are the units of signaling (p. 136)." "A form is often said to *express* its meaning (p. 141)."

*Markey.* Since Markey's general theoretical standpoint stems from Mead (123), there is considerable concern with language as social process which gets its meaning when the hearer "takes the role of the other individual" (121, p. 401) but its substitute-stimulus value is indicated in the following definition: "A language habit is a word stimulus or action stimulus, which is a substitute for or symbolic of a bodily habit or process, and an absent stimulus. . . . The substitute character in regard to these two factors is an important consideration of the definition." Markey has elaborated his symbolic theory further within a somewhat freely-interpreted conditioned-response framework in *The Symbolic Process* (122).

\* Note the familiar resemblance to the "form-content" problem in the mentalistic procedure and the faint traces of "expression." Here follows another nugget which serves to connect up Bloomfield's refurbished approach with the now forsaken expression theory: "The mental processes or internal bodily processes are known to each one of us only from speech-utterances and other observable actions (as if there were non-observable ones!). Since these are all we have to work with the mentalist in practice defines meanings exactly as does the mechanist (11, pp. 143-144).

*Hunter.* For Hunter (83) language is symbolic behavior which consists of responding to an absent object through a substitute stimulus of any sort. Its operation is similar to that of the kinesthetic cue for an animal in the delayed reaction experiment the function of which is that "of a *symbol*, because it enables the animal to react to an absent object in a selective manner, although it—the cue—has not been continuously present. This means that *we define a symbol as any process which is a substitute, which can be recalled if it ceases to be present* (p. 67)." The indirect nature of the response to the absent object is at once apparent in Hunter's formulation. One should also point out that included in language is *any* such indirect response, whether stimulated by an internal or external stimulus. Verbal behavior, one form of language, reaches its highest development in man and enables him "to behave with reference to absent objects in a way that makes science, religion, and the arts possible" (83, p. 330).

*Dockeray.* Dockeray (40, p. 142) follows Weiss in acknowledging language to be a form of stimulation of others as well as a response, and builds his machinery on a conditioned response base. A child, in the process of learning the language response, "milk," is described as follows: "The child sees milk; he hears the word, *milk*. He drinks milk and again hears the word, and so on, until *milk* has been definitely established as a symbol for a particular thing (p. 505)." There is here a first clear presentation of the symbolic relationship between word and absent object, which indicates the serial response of an individual to the word symbol, then to the object for which it stands—a relationship, it will be recalled, that Watson's formulation required but lacked.

*Dashiell.* According to Dashiell (34), language is a form of symbolic, social behavior in which words "stand for and represent" (p. 515) absent objects, thus extending man's behavioral space time. An example (Fig. 132, p. 512) shows Tom Sawyer signalling to Huck Finn, a symbolic stimulus which elicits a retreating response to the absent-but-fast-approaching-Aunt stimulus on the other side of the fence.

How the printed word "ORANGE" may be reacted to in a sign-and-thing signified relationship is described as follows:

... reacting to the black-marks-on-white is not reacting to some larger whole of which they are a part, as would be the case in recognizing a car by its noise or a burning pudding by its odor or an orange by the feel of it in a Christmas stocking. The child is responding to something with which the pattern of black marks has been artificially associated and for which it then has come to function as a substitute. It is not a full and complete substitute, to be treated as the original object or situation would be treated; it is always handled as in-relation-to that other (p. 461).

*Ruch.* While no definition of language behavior is given, enough evidence of Ruch's (148) behavioristic leanings is given in a few scattered statements. Again, the child is said to learn the names of objects by a conditioning process.

Eventually the child will be able to execute simple commands, such as "Bring me the doll from your basket." At this point we can definitely say that symbolic behavior is present. The child who hunts for the absent object is responding symbolically. The physically absent object is represented by some present symbol, and another symbol serves to direct the child toward the absent object" (p. 618).

*Carroll.* In a recent issue of the *Psychological Review*, Carroll (24) has outlined a sign-theory of language with the intent of doing justice to the work already done in formal linguistics (e.g., the speech unit, regularity, rate of occurrence, et cetera) and integrating these facts with systematic psychological theory. Toward this end, he adopts Morris' *semiosis* and extends his *pragmatics*. Peirce's *token* and *type* furnish models for *response* and *response type*, *response type* also being connected with Bloomfield's "speech community" (possessors of a cluster of "response-types").

Out of these components Carroll develops a symbolic theory which is best summarized in his own words.

A linguistic response subsumed under a linguistic response-type must first of all play a role in a sign-situation such that "something takes account of something else mediately." Secondly, the sign-situation must be such that an organism deliberately intends another organism to respond to the sign *as* a sign. Consequently, the response must be one which can be produced either directly or indirectly by mechanisms.\* Thirdly, the linguistic response must either itself be a sign, or it must form a segment of a sign. (This would include phonemes). Fourthly, there must be some social agreement among the members of a speech community with regard to the sign. Finally, the linguistic response-type must exhibit certain static and dynamic properties as a recurrent uniformity in the speech behavior of a number of individuals (p. 118).

### *Interbehavioral Language Theory*

Rejecting all preceding Mentalistic (Body+Mind) and Behavioristic (Body-Mind) theories, Interbehavioral language construction builds within a general framework in which, except for degree of complexity, an organism's responses to stimulus objects are treated as *naturalistically* as gravitational and chemical interactions.

\* Carroll would include within language such acts as an organism ringing a doorbell (p. 106), "the conventionalized choreographic movements of the modern dance" (p. 111) or "the deliberate wearing of one's hat at a rakish angle to suggest, say, the journalistic nonchalance of the city desk" (p. 112).

Language responses are segregated from other classes of behaviors when it is noted that the former are *bistimulational*. Viewed from the standpoint of the speaker, the organism concurrently *responds* to the hearer (the auxiliary stimulus) *and* the object talked about or referred to (the adjustment stimulus). On the side of the hearer, we see a similar bistimulational response involving the hearer's response to the *speaker's action*, which may be vocal, manual or other behavior (the auxiliary stimulus) and the thing talked about (the adjustment stimulus). There is no connection with theories that assume an "expression of ideas or thoughts" from one person to another; nor is the speaker's action *reified* into a series of symbols with which the hearer reacts *successively* and *which refer him* to objects (present or absent) as posited by both expression-symbol and behavioristic substitute-stimulus theories.

Furthermore, language is referential action in which the speaker *refers* to, and the hearer *is referred* to, some object, person or situation. Of course, an individual can talk to himself, or to put it another way, he can serve as his own auxiliary stimulus, and if he should talk to himself about himself, he also functions as the adjustment stimulus in such a situation. Typically, however, language is interpersonal and communicative.

In studying communicative language we must differentiate between the transmissive and receptive forms. The former constitutes linguistic action which serves as a stimulus for the language action of another person. Receptive language, on the other hand, is a definite response to transmissive language, either of the vocal auditory type (language heard) or of the graphic visual form (language read) (101, pp. 301-302).

Since psychological language must necessarily constitute someone's behavior, this feature distinguishes it from non-psychological language. When a linguist deals with transcriptions of an individual's verbal patterning, he has a *symbolization* (alphabetic, phonetic, phonemic) of that individual's buccal action *at the time* he did perform a linguistic response; but, obviously, such a record is not *the response*, because when the individual performed that linguistic behavior, it was a bistimulational behavior segment that involved an auxiliary and an adjustment stimulus as was indicated above. This point is emphasized because of the common misconception that has lead to an identification of written records with linguistic behavior. Kantor (100, p. 305) has suggested the terms "morphological" and "functional" language to distinguish "thing" language from language behavior. For example, refined analysis (102, p. 451, 104, p. 325) has lead to a distinction between symbolic behavior in which a verbal symbol (such as a calendar pad notation) serves as a



substitute stimulus to elicit a response to an object present or absent. The individual's response in such an instance would be *serial* or *successive*—first to the substitute stimulus, then to its designatum or referent. The verbal nature of the note on the calendar pad would be incidental, too, since any kind of *object* often recalls someone or something. But the distinguishing feature between verbal symbolic behavior and linguistic responses is the *succession* of response of the former and the *concurrent* bistimulational factors in the latter.

*How the Various Viewpoints Handle a  
Given Speech Occurrence*

Space restrictions prevent an elaborate analysis of Interbehavioral language theory. It would, perhaps, be more to the point to conclude

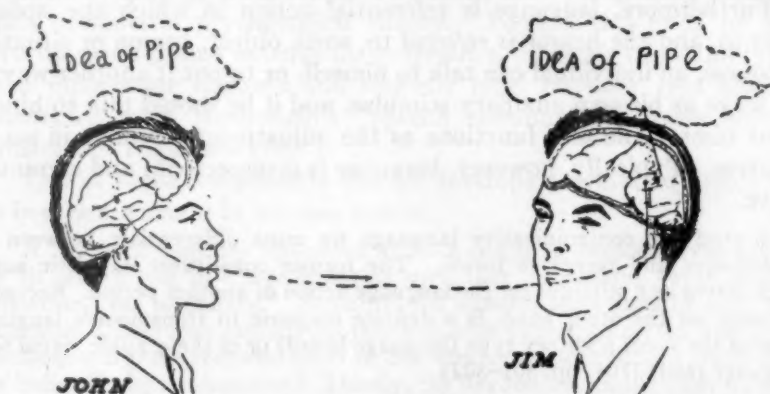


FIG. 1. THE EXPRESSIONISTIC THEORY'S ACCOUNT OF A LANGUAGE SITUATION

A language event in which John asserts, "There's your pipe." Diagrammed in accordance with expressionistic theory which maintains that the language event starts with certain ideas, desires, or feelings in John's "mind" which are transformed into neural impulses to the organs of speech. Then sound waves are propagated from John's mouth to Jim's ears by a "purely physical process." From this point on, the procedure is in inverse order to that occurring in John. There is first a physiological transmission of the acoustic image to the brain and finally a "psychical" association of this "image" with the corresponding "idea" of the pipe. Note that the series of events includes psychical-physiological-physical-physiological-psychical phases with gaps between each. (For other examples cf. Pillsbury E. Meader, Stinchfield, Gardiner, and Gray.)

by taking a concrete, speech occurrence and to show how the three chief types of construction would describe such a behavior situation.

Assume a situation in which John and Jim are customarily seated in the same room after dinner. Jim is looking for his pipe, John sees him

searching for it and says, "There's your pipe" (indicating the pipe on a nearby coffee table).

*A Mentalistic Version.* According to the Expression theory, John sees Jim searching for his pipe (Fig. 1). Knowing where it is, he "wants to convey the idea" of its location to Jim. Immediately, certain associated word-images are aroused which start motor impulses that put mouth, tongue, laryngeal and other musculature into action that results in articulation. A series of sound waves passes from John's mouth to Jim's ears, where they are translated into neural impulses that go up to the brain, where further translation results in an arousal of an idea in Jim's mind that corresponds to the original one in John's mind. Fig. 1, is intended to bring out the never-to-be explained gaps in the chain of

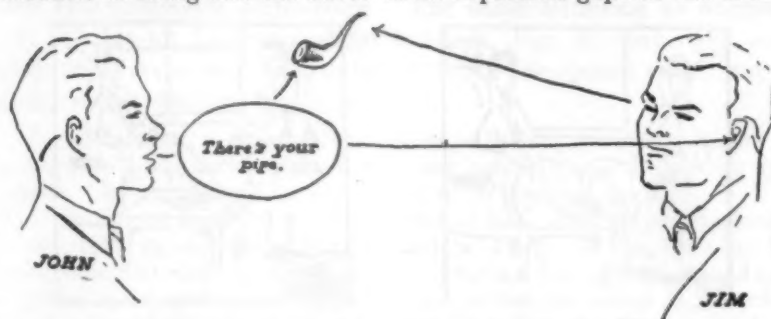


FIG. 2. HOW THE SYMBOLIC THEORY HANDLES A LINGUISTIC EVENT

The occurrence during which John addresses the remark, "There's your pipe." to Jim (same event as represented in Fig. 1) is here described as a symbolic situation. John's function is reduced to that of a *symbol-producer* and his speech *action* reified into a series of *things* that serve as stimuli for Jim. From this point on, the story may follow one of two courses: a. Behavioristic account—Jim reacts to the sounds then to the things for which they stand (i.e., the action involved is serial or successive). b. Mentalistic description—Jim reacts to the speech symbols, which arouse "thoughts" of the things to which they refer, which story would necessitate a diagram like that shown in Fig. 1. The symbol- here are only *indirectly* connected with their referents via "the mind." (Exponents of a: Watson, Weiss, Exponents of b: Ogden & Richards, Sapir & others.)

events which are alleged to be psychical→physiological→physical→physiological→psychical in turn.

Another Mentalistic account (Fig. 2) would vary the account slightly from the preceding except as a matter of emphasis. With the expression phase assumed but not developed, it would *reify* John's action into a series of as-if-frozen symbols. In other words, John's role in the speech event is quite incidental, being that of a producer of symbols. Now, Jim reacts to these products or symbols (according to Sapir, even to such



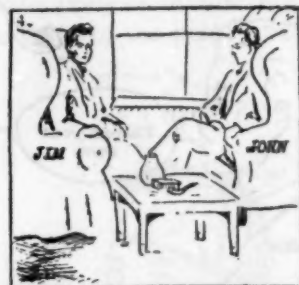
1. John looks out of window. Jim searches for his pipe. Unstimulational, non-linguistic behavior in both cases.



2. John sees Jim searching for his pipe. This illustrates interpersonal action on part of John but behavior of each is still unstimulational and non-linguistic.



3. John says, "There's your pipe," (or he can merely point). In this case John performs a *bistimulational* (language) action because he is at the same time in contact with Jim and pipe. This is a *referor* action.



4. Illustrating Jim's *referee* language behavior. *Bistimulational* also because Jim interacts with John's speaking action and pipe in the same behavior segment.



5. Jim says, "Thanks," or he may grunt, "uh huh." The first is only morphological language; the second, verbal gesturing. Neither is bistimulation of referential.



6. John again looks out of window. Jim manipulates pipe (effective action). Behavior of each is unstimulational, non-linguistic.

### FIG. 3. AN INTERBEHAVIORIAL DESCRIPTION OF A LANGUAGE ACT

The same even as that shown in Figs. 1 and 2 is here described but only the behavior segments shown in #3 and #4 constitute language behavior.

segments as prefixes and suffixes) which arouse "ideas" in his mind as above.

*A Behavioristic Explanation.* Fig. 2, may equally well illustrate the behavioristic account of the same speech happening since the psychic phases demanded by Mentalistic theories have been omitted in the diagram, but there are certain common features, namely, John's somewhat inferior role as symbol producer and Jim's serial action in his response to the symbols or signs (substitute stimuli) and then to the pipe for which they stand. Such accounts have been offered in an incipient form by Watson but have been more fully developed by Dashiell, Dock-eray and Carroll among others.

*An Interbehavioral Description.* The same situation as suggested above permits an Interbehavioral analysis (Fig. 3) that rules out a number of behavior segments only related to the speech event but not themselves constituting language action.

1. If, for example, in Scene 1 John should be looking out of the window at a blooming rose bush, he would be performing unstimulational behavior (non-linguistic). The same would be true for Jim in his search of the pipe.

2. If, in turning from the window, (Scene 2) John sees Jim (still looking for his pipe), John is performing interpersonal behavior, but the action of both is still unstimulational and non-linguistic. John's action will be followed in the next behavior segment by a linguistic response but his action in the present behavior segment should not be confused or identified with it.

3. John says, "There's your pipe," or he points with his finger or nods in that direction with his head. In any case we have transmissive or referor action. This is the first language behavior segment to occur in the situation. (Scene 3). It is a linguistic response on John's part because it is *bistimulational*—that is, John's behavior cannot be adequately handled without at the same time including Jim (auxiliary stimulus) and pipe (adjustment stimulus).

4. In Scene 4 Jim also performs a linguistic response because within the same behavior segment he has psychologically interbehaved with John's *action* (auxiliary stimulus) and to the pipe (adjustment stimulus). Jim's action is not reified into a symbol product, the inadequacy of which would be most apparent in the case of John's gesturing. It is pertinent to ask: for what could the gesture be a symbol?

5. In Scene 5 if Jim should say, "Thanks," this would be morphologically language but he would not, in this case, be referring to anything, nor would Jim from his side be referred to anything. Since it would not be bistimulational behavior, it would not be referential action but would more closely approximate a "verbal formula" or a conventionalized verbal gesture.

6. Assuming John to return to his gazing out the window and Jim to the manipulation of his pipe (an *effective* response) we would again in Scene 6 have unstimulational and therefore, non-linguistic behavior.

The tool which permits the differentiations of behavior indicated above is the "behavior segment" feature of Interbehavioral analysis.

As a result, behaviors showing a superficial resemblance at first blush show marked differences upon dissection. Furthermore, the inclusion of data covering the full range of linguistic behavior data as well as the coherence and precision of statement achieved within this framework contrast markedly with the sterility of previous theories. Finally, Interbehavioral language theory affords opportunity for the formulation of linguistic investigation that would go beyond the word count studies so endlessly inspired by the Expression and Symbol theories.

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## OBJECTIVE MEASUREMENT OF CLINICAL STATUS IN PSYCHOPATHOLOGICAL RESEARCH

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What therapies are most effective for the various functional disorders? This problem has long been a pressing one in applied psychopathology, and will become even more important as new treatments are developed. Its solution requires careful experimental planning and the use of control groups and appropriate statistical techniques. Perhaps the most important requirement, if the relative values of different treatments are to be validly determined, is that treatments be investigated repeatedly with comparable groups and results be reported in comparable terms. Unfortunately, standard means for describing research populations and their degree of clinical improvement have not yet been established. As a consequence, experimental investigations of treatments have been handicapped. It is difficult for the investigator to build up equated research groups either for the purpose of determining the degree of superiority of a treatment over routine hospital care or for determining the relative values of two different treatments. The estimation of differences in clinical improvement between research groups is also beset with difficulties. Even the evaluation of a single treatment administered to a single diagnostic group by different investigators has become an uncertain task. Jessner and Ryan (36), for example, note that the statistics of insulin treatment as reported in the literature for schizophrenia vary from 70 per cent recovery to 20 per cent improvement, while some studies find even poorer results than in untreated cases. Such widely divergent results are difficult to reconcile so long as research groups, bearing the same descriptive labels, vary to an unknown extent in initial clinical status or subsequent degree of improvement.\*

It seems that sound experimental studies of treatment in the functional disorders must first rest upon firm descriptive foundations. The present paper, therefore, will be devoted to

1. an analysis of present day description of clinical status in psychopathological research,
2. a tentative formulation of the salient features of a desirable descriptive system, and

\* Other possible variables influencing these treatment figures and requiring control are (1) the mode of application of the therapy, (2) the influences of adjuvant therapy, (3) the period of follow-up study, and (4) the investigator's bias.

3. an evaluation of the role clinical psychology can play in contributing to the development of such a system.

#### PRESENT DAY DESCRIPTION OF CLINICAL STATUS

Perhaps the most basic type of study in treatment research is one designed to determine the degree of superiority of a therapy over routine hospital care. Such a study must meet at least two major requirements; first, its experimental and control groups must be comparable and second, its standards for assessing clinical improvement must coincide. Obviously, similar requirements would hold for studies designed to determine the relative values of two different treatments or when different investigators wish to compare validly results obtained with the same treatment. Let us examine these requirements more closely and the extent to which present day methods of describing clinical status help fulfill each of them.

##### *Describing Research Populations*

Experimental and control groups, to be comparable, must be matched as closely as possible for (1) capacity for spontaneous improvement (or prognostic outlook) and (2) qualitative status. The efficacy of a treatment is measured in terms of the degree to which improvement under this treatment exceeds the degree of spontaneous improvement to be expected with only routine care. Consequently, if experimental and control populations are to be compared with respect to recovery rates, they ought to be initially comparable in their capacity for spontaneous improvement. Also, if the value of a given treatment for a given type of patient is to be determined validly, both control and experimental groups must equally represent the qualitative status in question.

*Capacity for spontaneous improvement.* Various symptomatological features (process symptoms, atypicality, elements of confusion, etc.) and background factors (duration of illness, type of onset, pre-psychotic personality, etc.) are related to capacity for spontaneous improvement (14, 54). In most studies, however, the prognostic outlook of treated groups is defined solely in terms of diagnostic category, duration of illness, or both. Although diagnoses bear some relation to prognosis, their limited reliability and validity (to be discussed in the following section) make them unsuitable indices of capacity for spontaneous improvement. Duration of illness, the single background factor most commonly used for designating prognostic outlook, can be considered no more satisfactory. Although there is considerable agreement concerning the relation between duration and outcome of illness, this rela-

tion is insufficiently reliable in the individual case. For example, Hunt, Feldman and Fiero (32) note that no improvement took place in 45 per cent of 166 schizophrenics ill for less than six months, while improvement occurred in 25 per cent of 330 cases with more than 18 months duration. It should also be noted that the estimation of duration of illness, based as it is on information obtained from relatives and friends, is frequently undependable and at best only approximate.

Capacity for spontaneous improvement, then, cannot be satisfactorily assessed in terms of any one or two gross factors. Rather a numerically graded evaluation of the total complex of factors bearing significant relation to prognostic outlook seems required. Until these prognostic aspects of research populations are rigorously and uniformly defined, comparison of improvement rates must remain a dubious procedure.

*Qualitative status.* Research workers describe the qualitative status of patients under treatment chiefly in Kraepelinian diagnostic terms. Kraepelin's classification, although it originally tended to clarify the definitions of the functional syndromes, at present lacks sufficient *reliability* and *validity* for satisfactory coordination of research. Diagnostic criteria vary markedly from hospital to hospital and from one psychiatrist to another. For example, Boisen (5) comparing the relative number of schizophrenic subtypes in Illinois with hospitals in other sections of the country found that whereas in Massachusetts 30% of the newly admitted schizophrenics are classed as catatonics, Illinois has only 2.7%. On the other hand, whereas Illinois places 45% in the hebephrenic group, Massachusetts finds only 15.5%. Myerson (47) points out that even when psychiatrists agree as to what constitutes a specific mental disorder they may still make inconsistent diagnoses because of the difficulties in correctly recognizing criteria symptoms (e.g., negativism, resistiveness, apathy) when they occur in individual cases. The consequent unreliability of diagnoses limits to an unknown extent the comparability of patient groups, particularly those treated by different investigators.

A description of the patient's qualitative status, to be most valid, must include all the patient's clinical trends and their relative intensities. Kraepelinian diagnoses, however, are not sufficiently refined for such comprehensive description. Functional psychoses frequently represent a mixture of schizophrenic and affective elements. Yet many research workers, treating the major functional psychoses as if they were demarcated entities each with its own pathology, ignore secondary clinical trends and force the case being considered into one or the other



major classification. They then consider, at least for research purposes, all cases under the same label as constituting a homogeneous disease entity.\* Referring to this procedure, Cameron (10) states:

Most of the confusion and the contradictions in the literature on functional psychoses can be traced to the wholly unjustifiable practice of treating the heterogeneous mass of major depressive and manic disturbances as if they really constituted a single illness, Kraepelin's "manic-depressive insanity," and of lumping together the schizophrenic disorders likewise as a homogeneous disease entity as though it were as uniform as chicken-pox.

The tendency to classify patients into one discrete category or another obscures not only the qualitative mixtures in clinical trends but also their relative degrees of manifestation. For example, one patient diagnosed "schizophrenia, catatonic type" may exhibit marked catatonic features, moderate paranoid trends and traces of hebephrenic symptoms, while another similarly classified patient, although equally catatonic, may be less paranoid and more hebephrenic. Gross, ungraded diagnostic terms conceal these sometimes significant differences in symptom intensity. Yet investigations have clearly indicated that individual differences in most psychological traits are distributed on a graded and continuous scale. Undoubtedly, most psychopathological trends are similarly distributed. Terms already exist for describing gross degrees of mania and depression, but these are rarely used to describe research groups. Furthermore, no equivalent terms of gradation exist for schizophrenia. Obviously, lumping patients together under the same diagnostic label who vary in the relative strengths of their clinical trends tends to obscure further their qualitative status.

### *Describing Clinical Improvement*

If recovery rates are to be validly compared, the same standards of clinical improvement must be applied to the different research groups under consideration. Unfortunately, the improvement categories customarily employed vary in use and meaning from one investigator to another. The term "improved," for example, has been used in one study (60) to mean symptom amelioration plus ability to adjust in the outside community, while in another investigation (48) it was applied to patients showing symptom amelioration *without* the ability to adjust on the outside. In some studies (48) the patient is classified as "recov-

\* It is recognized that many research workers are well aware of the shortcomings of this procedure and consider it a necessary convenience. Although the author sympathizes with the need for this expediency, it must be realized that such a procedure represents a barrier to future progress.

ered" if he returns to his prepsychotic status, while in others it is also required that he develop insight regarding his illness (16, 58). Some psychiatrists do not consider any of their patients capable of recovering from schizophrenia (60).

Judgments of clinical improvement lack not only a standard terminology but also objective methods of derivation. Their reliability and validity are therefore doubtful. Cohen, Malmo and Thale (17) appear to recognize this when they state:

The lack of acceptable objective methods of evaluating the nature and changes of activity in mentally ill patients is probably one of the greatest obstacles to the valid interpretation of psychiatric data. The usual clinical judgments of "improved," "unchanged," or "worse" are too frequently derived from anecdotal data, offered by ward nurses and attendants. It is true that much ward personnel has the inestimable advantage of observing given patients for prolonged periods. But it must be admitted that, failing checked objective methods of observation, their opinion (necessarily a composite one) may be formed in a quite capricious fashion.

A valid statement of changes in intensity of disorder would obviously depend upon adequate descriptions of qualitative status both before and after treatment. Unfortunately, the difficulties that apply to present day description of initial qualitative status obtain equally in describing changes in status.

#### OUTLINE OF A DESIRABLE DESCRIPTIVE SYSTEM

From the foregoing analysis it seems clear that many of the difficulties involved in the evaluation of a treatment's efficacy can be traced largely to the lack of objective, standard descriptions of clinical status. This lack stems from the emphasis of present day descriptive methods upon crude, discontinuous categories instead of well-defined, measurable variables. True, the rough qualitative mapping out of the functional syndromes was a necessary stage in psychopathology, but the time now seems ripe, and the need urgent, for attempting a transition to more precise, objective and possibly more fundamental modes of description. Such a desirable descriptive system would involve the following procedures:

1. The functional psychoses would be described by a minimal number of clearly defined, qualitative components (or dimensions) of abnormality.\*

\* It is assumed that such basic variables can be isolated. Studies supporting this assumption will be presented in a later section. Future research must decide whether these components would be identical with present day syndromes or with more fundamental underlying variables.

2. These primary components would be elicited and measured by objective tests.

3. Analysis of clinical status would require measurement of each component.

4. Both capacity for spontaneous improvement and qualitative status would be described in terms of *cross-sectional* patternings of the same component strengths.\*

5. Clinical improvement would be described in terms of *longitudinal* changes in these patterns.

In such a multidimensional system, the descriptive questions would not be merely: "Is the patient schizophrenic or manic-depressive, or is he in a partial or full remission?" Rather they would become: "*To what extent does the patient exhibit each of the basis components of functional disorder, and how do his present component strengths compare with what they were before treatment?*" This comprehensive mode of description would automatically indicate capacity for spontaneous improvement, better provide for "mixed" syndromes and relative intensity of clinical trends, and would lay a more accurate and reliable basis for defining clinical improvement.

Clinical psychology, long concerned with the problems of exact measurement, may be of considerable aid in identifying and measuring primary components of abnormality. Psychological testing and methods of "psychostatistical" analysis appear to be particularly promising and appropriate tools for attacking this problem. The essential features of psychological testing are

1. the devising of standard situations considered representative of broader areas of behavior,
2. the quantitative measurement of individual differences in reactions to these situations, and
3. the comparison of the individual's performance with previously established norms.

By *psychostatistical* analysis we refer to those factorial methods which have been developed for deriving from many measures the minimal number of variables necessary for description of behavior. In the following section we shall survey a number of psychological studies containing significant hints as to how these and other approaches may

\* From an ahistorical view, only contemporary qualitative status can determine capacity for spontaneous improvement. Background factors are of significance only insofar as they indicate the present condition of the patient. From this point of view, objective tests are legitimate and probably the most desirable means of assessing capacity for spontaneous improvement. In the course of developing the above system, however, background factors will for some time probably supplement objective tests.

contribute to the development of a more useful and objective descriptive schema.

### PSYCHOLOGICAL MEASUREMENT OF CLINICAL STATUS

Psychiatrists have long recognized the need for exact methods of description. According to Hollingworth (28), Kraepelin himself was much concerned with replacing vague, subjective judgments by precise, objective records of performance elicited under controlled conditions. Although he recognized the significance of the more complex aspects of personality, he preferred to limit his studies to functions he could measure, such as work capacity, learning rate, retentivity, etc. Since the Wundtian psychology of Kraepelin's time, however, advances in testing theory and technique have opened up the prospects of extending Kraepelin's admirable experimental program to include the deeper, more complex aspects of abnormal behavior. A vast literature has accumulated dealing with the application of psychological techniques to the psychiatric disorders. Although differing widely in procedures, aims and theory, these studies have generally emphasized exactness and objectivity. A comprehensive summary of most of these investigations can be found in Hunt (30), Hunt and Cofer (31), and Brody (8). The bulk of these psychological studies can be classified as follows:

1. Studies concerned with predicting outcome of shock therapy and bearing on the problem of assessing capacity for spontaneous improvement.
2. Studies in which attempts are made to differentiate qualitative types of disorder.
3. Studies mainly concerned with distinguishing groups varying in intensity of disorder.
4. Studies bearing on the problem of describing clinical status economically by means of basic variables.

Below we shall consider the most promising studies of each type and the implications they carry for the problems of this paper.\*

#### *Determining Capacity for Spontaneous Improvement*

Patients suffering from the functional psychoses differ in their capacity for spontaneous improvement. Can these differences in capacity be assessed by psychological tests? In the absence of studies bearing directly on this question, let us examine related investigations attempting

\* For many of the studies one might question whether the results would have been the same if they had been carried out by other investigators using different diagnostic criteria. Despite this qualification, it is felt that the total body of results strongly supports the contention that a more objective, exact system of clinical definition and measurement can be derived by means of psychological techniques.

to determine capacity for positive response to shock treatment:

1. *A number of investigators have found the Rorschach test useful in predicting response to shock treatment.* Halpern (23) administered the Rorschach to 17 schizophrenics before insulin therapy. According to the test results, the group contained two types of patients. One group presented a picture of rigid personality and general nonproductiveness, incapable of emotional response and showing little capacity for identification with others. These individuals responded poorly to treatment. The other group, representing patients who displayed receptivity to emotional stimuli and a capacity for empathy, attained satisfactory adjustment for a period of a year or more. The three Rorschach signs differentiating these two groups would have enabled correct prediction of treatment response in 14 out of 17 cases.

On the basis of "blind" analyses of Rorschach records, Piotrowski (51) was able to predict improvement resulting from insulin treatment correctly 88.3% of the time, i.e., in 53 out of 60 cases. With added experience he found that more precise definition of prognostic signs would have yielded correct predictions in 93.3% of the cases, i.e., in 97 of 104 patients. He concluded that the greater the degree of intellectual regression the less the chances of improvement, while the greater the degree of emotional regression the better the chances of improvement. He felt that the Rorschach distinguished between these two types of regression with greater precision than would have been possible without its use.

Eisner and Orbison (18) administered the Rorschach to catatonics and manics before metrazol treatment, once under normal conditions and then again in the sodium amytal twilight state. They found that certain effects of sodium amytal on Rorschach performance were of prognostic significance. For example, the drug's affect-releasing action, as reflected in increased color scores, was greater in the more improved than in the less improved groups.

Morris (46) found six Rorschach signs which, if applied to selection of patients for metrazol treatment, would have enabled him to yield an improvement rate of 78%, a gain of 34% over clinical selection alone.

2. *Concept formation tests have also been found to be of prognostic value.* Bolles, Rosen and Landis (6) report that performance of 19 patients on a modified Vigotsky test of concept formation, the Weigl test and the BRL sorting test showed closer relation to outcome of insulin treatment than did the duration of illness.

Zubin and Thompson (76) found a correlation of .68 between pre-treatment test performance on the Weigl, BRL and Vigotsky and immediate outcome, and a correlation of .50 between combined background factors and immediate outcome in a group of 49 patients treated with insulin shock. If both criteria had been applied in the case of the insulin patients and only those patients who obtained favorable ratings on both had been selected for therapy, the probable improvement rate for immediate outcome would have been 95.4% and for outcome on follow-up 76.3%.

3. *Combinations of objective tests have been found useful.* Wechsler, Halpern and Jaros (71) found that a test battery consisting of a vocational interest blank, tests of counting by 3's and naming words in three minutes, a similarities and a directions test not only corresponded well with psychiatrists' estimates of



effects of treatment, but also gave a correlation of .73 and a percentage of correspondence of 87 with a clinical appraisal of the patient's condition 6 to 18 months after treatment.

4. *Attempts at predicting treatment response by means other than objective tests have also been successful.* With various symptoms and background factors as prognostic criteria, Chase and Silverman (15) assessed the probable outcome for each of 150 patients treated with metrazol or insulin shock. These assessments were compared with later results of treatment. Of 43 cases in which the prognosis was considered good, 33 (77%) showed remissions. On the other hand, of 74 cases in which a poor outcome was predicted, 63 (85%) remained unimproved.

The foregoing type of statistical prediction may be considerably facilitated by use of a rating scale such as that devised by Wittman (75) which weights the various symptoms and background items considered prognostic. According to Wittman, this scale gave a definitely more accurate prediction of outcome following shock therapy than the average medical staff member's judgment.

The above studies carry the following implications for our problem:

1. Capacity for spontaneous improvement can probably be assessed with a relatively high degree of objectivity and quantification.

2. Development of an objective index of capacity for spontaneous improvement would require the establishment of a test battery correlating with spontaneous changes in clinical status rather than with responses to specific treatments. The mode of analysis necessary for establishing such a battery would be identical with that employed by the above studies, i.e., by finding the test items in the improved group which originally differentiated them from the unimproved group.

3. Only a negligible number of psychometric techniques have been studied from a predictive viewpoint. It appears likely that many of the tests to be suggested in the following sections for eliciting and measuring qualitative deviation would also have prognostic value.

4. It seems likely that certain drugs, such as sodium amytal, modify test performance in such a manner as to disclose existing trends more sharply or reveal hitherto latent capacities. Administering psychological tests to patients under the influence of such drugs, following the method of Eisner and Orbison (18), may increase the prognostic value of these tests considerably.

### *Determining Qualitative Types of Disorder*

There are different qualitative syndromes among the functional psychoses. Do these various types of psychotics behave distinguishably on psychological tests? Do they show qualitatively different performance patterns? Let us examine the available evidence:

1. *Projective techniques have been useful in revealing the qualitative character and organization of various functional syndromes.* Klopfer and Kelley (40) sum-

marize the characteristic patterning of Rorschach signs which have been found for schizophrenic, depressed and hypomanic states. The fine differentiation of schizophrenic subgroups is still in the beginning stage "... although these types may be recognized in the individual case." The Thematic Apperception Test enables identification of psychiatric diagnoses with fair accuracy (24). The Szondi technique, in which the subject picks those he most likes and dislikes from six series of photographs of abnormal and criminal types, seems to have good potentialities (53). Angyal (3) finds that schizophrenics, reproducing tachistoscopically exposed letter combinations, reveal differences among themselves which are related to hebephrenic and paranoid characteristics.

2. *From Lewinian experiments comes objective evidence of the schizophrenic's apathy.* According to Hunt and Cofer (31), Stumbur found that unlike Zeigarnik's normals, schizophrenics failed to recall more "uncompleted" than "completed" tasks. Rickers-Ovsiankina (55) failed to find schizophrenics showing the normal tendency to resume "uncompleted" tasks. Escalona (19) found that normals, manics and depressed patients shifted characteristically in their level of aspiration under the effects of success and failure.

3. *Studies of prompted drawings of psychotics in terms of such characteristics as gestalt properties, color, movement and restraint have been found indicative of various functional syndromes.* These investigations have been well summarized by Anastasi and Foley (1, 2). Mira (44) claims he can deduce various diagnostic characteristics when changes of direction and magnitude are introduced into a subject's drawings as he tries to copy from a model.

4. *Temperamental factors isolated by factor analysis of groups of simple tests have been found to be related to functional syndromes.* Possibly the most systematic attempt at objective testing of abnormality has been undertaken by a group of English experimentalists under the leadership of Spearman (69). By means of factor analysis of groups of simple tests requiring a minimum of intelligence, they have isolated a number of temperamental factors ("p" or perseveration, "f" or fluency, etc.) which in combination with "g" have been found to differentiate between normals and various diagnostic groups. Characteristic combinations of "g," "p" and "f" test scores have been found for manics, depressives, hysterics, and schizophrenics.

5. *Inventories and questionnaires have been adapted to the discrimination of clinical syndromes.* The Humm-Wadsworth Inventory (29) and the Minnesota Multiphasic Scales (26) are outstanding examples of the questionnaire type of test which has been adapted to the discrimination of clinical syndromes and carefully validated by means of control groups.

6. *Analysis of abnormal patterns and scatter on standard intelligence tests appears promising.* Brody (8) and Hunt and Cofer (31) summarize a variety of psychometric studies designed to discover characteristic abnormal patterns of performance on standard intelligence tests. Recently Schafer and Rapaport (61) have outlined several new conceptions of scatter which appear to give clear expression to significant variations in the Wechsler-Bellevue score patterns of different clinical groups. Although no reliable generalizations can yet be made, many of these studies appear very promising.

7. *A variety of other measures of behavioral functioning or adaptation have interesting possibilities.* Reversal of the "fatigue effect" tendency with continuous work, slowness of perceived fluctuations in ambiguous figures and possibly

certain alterations of the patellar reflex have been pointed out by Hunt and Cofer (31) as specifically characteristic of manic-depressives. Studies of behavioral correlates of the moods of elation and despondency (e.g., speed and quality of associations, number of spontaneous remarks, time to make decisions) offer interesting possibilities for objective measures of mania and depression (20, 37). Hausmann (27) reports a method for objectively measuring "thinking difficulties." He believes this method can eventually be used for differentiating depressive and schizophrenic reactions at an early stage.

8. Wertham (73) summarizes many ingenious tests, yet to be validated, which have been invented for discriminating cycloids from schizoids. By means of these tests it has been found, for example, that cyclothymics are more sensitive to color than to form, and are more distractible than schizothymics.

The above studies carry the following implications for our problem:

1. The development of tests for eliciting and measuring qualitative syndromes of functional abnormality is still in its infancy and appears to be a difficult but not impossible task.

2. A wealth of experimental techniques have shown varying degrees of promise. The crying needs of most of these techniques are for validation, fuller standardization and more objective scoring schemes. Many need to be repeatedly applied to groups of patients covering the whole range of functional disorders and correlated with ratings of clearly defined clinical characteristics rather than with psychiatric diagnoses.

3. The most promising tests are those which disguise their true purpose, tap central regions of personality, permit multi-scoring, and require the subject to perform a specified task rather than report about himself.

4. A battery of tests, rather than any single measure, is most likely to be effective in qualitative diagnosis. Such a test battery would probably require (a) the establishment of critical scores for each subtest, i.e., points at which qualitatively different groups were maximally discriminated from each other, and (b) the development of diagnostic score-patterns.

#### *Determining Quantitative Degree of Disorder*

We have seen that the functional syndromes can probably be differentiated qualitatively by means of objective tests. Now we turn to the problem of intensity of disorder. If we conceive of normals, neurotics and psychotics as falling at different points on a normality-abnormality continuum, can test scores correctly place these different groups on this continuum? This is perhaps the simplest demand that can be made of psychological tests, i.e., that they grossly differentiate groups of normals from groups of patients varying widely in degree of mental illness, regardless of type. It is obvious that most of the tests listed in the fore-

going section can probably meet this requirement, since they elicit not only different qualitative aspects of behavior but also measure their manifested intensity by means of graded scores. Below are listed additional studies which have centered specifically on this problem:

1. Normals, neurotics and psychotics, when ranked according to their weighted scores on a test battery which included the Rorschach, Spearman factor tests, Bernreuter, and Wells word association, were found to be in approximately the same relative position as that determined by clinical appraisal (41).

2. Cattell (11, 13) claims that his Cursive Miniature Situations test "... probably distinguishes between normals and psychotics more completely than any other test, motor or verbal, yet tried for that purpose." This test requires subjects to circle and cross out specified marks on a paper strip moving beneath a small aperture.

3. Penrose and Myers (49) have found that abnormal score patterns on a group intelligence test ("General Examination-M") distinguished between psychotic and non-psychotic subjects in about 75 per cent of the cases. They suggest this test pattern method as a screening device for identifying psychiatric suspects.

4. The Multiple Choice Rorschach (25), Rorschach "instability" scores (59), word association tests (38, 43, 72), Bernreuter test profiles (62), and a food check list (70) have also been found to discriminate to some degree between normals and various deviating groups.

5. Two *normal* groups representing extremes in camp adjustment were satisfactorily differentiated by a composite profile derived from three "stress" tests (57). Obviously, tests which distinguish normal groups varying in adjustment are even more likely to distinguish between more extreme deviant groups.

6. Tests of deterioration (4, 65, 66) and the more peripheral functions such as reaction time (34), tapping rate (63), and tachistoscopic perception (3) have also been found to be associated with judged severity of disorder. Rodnick and Shakow (56) studied reaction time to regular and irregular preparatory intervals of various lengths "... as a quantitative measure of the ability of the schizophrenic to reach and maintain a high level of preparation in meeting recurrent environmental stimulation." A composite index based on differences between patients and normals separated the two groups with practically no overlap.

7. Psychological tests have also discriminated between degrees of intensity of illness in the *same group tested at different times*. Differences between Rorschach responses (33, 39, 50), perseveration (67), concept formation (76), and intelligence test patterns (22, 35) when compared before and after treatment have shown positive correspondence with clinical improvement. A battery of tests including the Rorschach and tests of intelligence, perseveration, orientation, concept formation, word association, etc. showed a "fair correlation" between the clinical psychiatric rating and psychometric rating, before, during and after insulin shock therapy (42).

The above studies carry the following implications for our problem:

1. Reliable psychological tests for measuring intensity of functional



disorder can probably be developed within the near future. Such quantitative measures, even if gross, would represent a considerable advance over present day methods of evaluating clinical improvement.

2. Tests confronting subjects with an *unpredictable sequence of stimuli and continual necessity for maintaining set and attention* appear to be particularly effective means for measuring gross intensity of illness. Examples of such tests are Rodnick and Shakow's (56) reaction-time set-up with irregular warning procedures and Cattell's Cursive Miniature Situation (11, 13), involving a continuously developing series of events in unpredictable succession.

3. Since no single test completely differentiates normal and disordered groups, a *battery* of tests tapping several levels of personality organization would probably be most successful in measuring intensity of illness.

4. Degrees of abnormality can probably best be discriminated by means of empirically derived *composite indices*, combining several weighted variables and analogous to those devised by Rodnick and Shakow (56) and Rodnick, Rubin and Freeman (57).

5. If tests are to be reliably applied to individuals, future studies must pay particular attention to the practical question of *overlap* between groups as well as to the significance of differences between group means.

#### *Determining Basic Variables of Abnormality*

The investigator who hopes to derive an exact and systematic description schema for assessing clinical status is confronted at the outset with a bewildering variety of clinical symptoms. Developing objective tests for each of these symptoms would be an almost insuperable task. Therefore, he may reasonably inquire: How can abnormality in the functional psychoses be described most *economically*? Are some symptoms more basic than others? Or do certain fundamental variables exist which underly all these diverse symptoms?

The various methods of factor analysis may provide a fruitful approach to these problems. These technical methods of correlational analysis aim to derive that minimal number of variables most adequate for the systematic and orderly description of personality. It is not within the province of this paper to discuss the pros and cons of the various systems of factor analysis, nor of the basic psychological and mathematical assumptions of the approach itself. Some of the more promising studies, however, which have adopted this general mode of analysis will be briefly described below:

1. *Orthodox factor analysis.* In a pioneering study, Moore (45) applied factor analysis to the intercorrelations of 40 ratings of psychiatric symptoms in 402 psychotics. He was able to extract 5 factors, which he called "catatonic,"



"deluded," "manic," "cognitive defect," and "constitutional hereditary depression." The possibility of determining basic syndromes by means of mathematical techniques was thus established.

Line and Griffin (41), in a search for basic measurable variables underlying mental stability, administered a large battery of tests to 43 subjects ranging in "stability" from mental hospital patients to graduate students. Analysis of the results indicated two factors in the scores on these tests, "objectivity" and "fluency." The investigators concluded: "If other factors are detected, and if they prove to be pertinent to the field in which we are interested, patterns of these factors as represented in the functionings of individuals may then be compared. Should these in any way correspond to clinical types, we may thus arrive at the point where factor-patterns give direction to the search for what constitutes the difference between types."

Cattell (12) has performed the largest scale factor analysis of personality measures to date. He has derived twelve primary factors which he has interpreted with reference to psychiatric syndromes. Although too early to judge, this monumental work may mark the first step towards the establishment of the type of descriptive system deemed desirable in this paper.

2. *Cluster or syndrome analysis* represents a more empirically obvious method of correlational analysis insofar as it merely aims to discover what correlation coefficients cluster together, i.e., intercorrelate positively among themselves and show negative correlations with all or nearly all other traits. It is assumed that these clusters represent true trait unities. By means of this mode of analysis Sheldon (64) has isolated three basic components of temperament (each represented by clusters of 20 traits), and has demonstrated their close correlation to the individual's basic morphological pattern. The relations between these components and the major functional disorders are being investigated.

3. Stephenson (68) has suggested an "*inverted factor technique*" for deriving personality types. This approach consists of obtaining the correlations between the scores of persons instead of tests. The factors obtained are not traits of personality but types of individuals, i.e., typical orderings of traits to which all persons of a type approximate. It is possible to give to each individual a score which indicates his degree of correlation with this "type-factor." Bordin (7) points out that this type of analysis permits more objective identification of factors than orthodox factor analysis: "Since one is now dealing with persons who have high and low factor loadings, the facts can be obtained in a public manner. Persons have many characteristics which are observable by all and on which all can agree. For example, they are male or female, they have been good or poor achievers, have lived in high level or low level economic environments, etc. These characteristics may then be related to the factor loadings by appropriate statistical procedures for indicating relationships and thus result in an identification or non-identification of factors. In essence, it represents a means whereby the best parts of the clinical and statistical methods may be combined."

The above studies carry the following implications for our problem:

1. With the aid of "psychostatistical" methods the derivation of basic components of abnormality appears to be possible.
2. This goal may be fulfilled by means of a systematic research

program which, in its simplest outlines, would involve the following steps: (a) Testing a large representative group of functional psychotics, upon admission and also after six months of routine hospital care, with a test battery sampling a wide range of behavior variables. (b) Rating this patient group before and after the six-month period on a large number of symptoms, ward behavior items and background factors.\* (c) Analyzing "psychostatistically" the network of functional relations existing among tests and ratings.

3. The coordinated design of the above research plan would permit not only investigation of basic variables underlying the intercorrelations between tests and ratings, but also supplementary attacks upon the problems of objective measurement of prognostic outlook and of clinical improvement.†

By employing psychological tests and "psychostatistical" methods in organized research programs a frame of reference may be eventually derived within which functional disorders could be classified and scaled. Once fundamental dimensions of deviation are discovered (be they those of Moore (45), Line and Griffin (41), Cattell (12), or others), we may then standardize objective tests for eliciting and measuring each of these dimensions in any given patient. Each patient would thus be described by a set of scores assigning him his quantitative status with respect to each variable. Such a descriptive system would conceive of functional abnormalities not as discrete psychiatric categories, but as patterns of relatively independent, numerically graded components.

The program outlined above will obviously require many years of concerted effort on the part of both psychiatrists and clinical psychologists for fulfillment. Yet the problems attacked are of such a fundamental nature that the time and effort would undoubtedly be well spent. As sound descriptive foundations are established, the frequencies of successes and failures for different treatments will be stated and compared with ever greater confidence. The values of various therapies will be more adequately determined. The way will be open to the establishment of statistical prediction tables listing in ascending order the efficacy of treatments for different types of patients—an achieve-

\* A number of suggestive rating scales of clinical behavior have appeared (9, 52, 74) but their use in psychiatric research has been all too infrequent. The author agrees with Sheldon (64), particularly for research in psychopathology, "... that in our struggles with the complex data of psychology we have neglected unduly the technique of ordering, classifying and scaling our materials through ratings of trained judges."

† The experimental program outlined above is only a tentative proposal. New developments in the field or individual circumstances may require appropriate modifications in this design of research.

ment which will raise the practice of applied psychopathology to a new level of effectiveness.

#### SUMMARY

1. Present day methods of describing clinical status lack satisfactory reliability and validity. The comparison and evaluation of treatment results will remain extremely difficult as long as research groups, bearing the same descriptive labels, vary to an unknown extent in their capacity for spontaneous improvement, qualitative status and degree of clinical improvement.

2. A desirable descriptive system would be characterized by a minimal number of abnormality components which could be elicited and measured by means of objective tests. Patients would be described in terms of their patternings of component strengths and shifts in these patterns.

3. Relevant studies involving psychological tests and "psycho-statistical" methods have been surveyed with a view to evaluating their potential contribution to the development of such a system.

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## A HISTORY OF THE DEPARTMENT OF PSYCHOLOGY AT THE UNIVERSITY OF CHICAGO\*

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When the new University of Chicago opened its doors on October 1st, 1892, there was not, nor was there for ten years thereafter, a Department of Psychology. In the Department of Philosophy, later known as the Department of Philosophy and Education, several courses in psychology were offered; and of its first 14 Ph.D. dissertations, four dealt with psychological topics. In 1900 the first doctorate in Education was awarded, and three years later, in 1903, the newly independent Department of Psychology granted its first doctorate, to John Broadus Watson.

### I. BEGINNINGS OF THE DEPARTMENT: HOUSING AND EQUIPMENT

Instruction in psychology, the first year of the University, was given in Cobb Hall, then the only lecture hall on the campus. After the erection of Ryerson Physical Laboratory in 1893-94 two rooms were provided for a psychological laboratory on the third floor, which were occupied for three years. The first description of its equipment mentions models of brain and sense-organs, and apparatus for vision, hearing, touch, and movement, and for time-measurement. With the erection in 1898 of the four Hull Biological Laboratories, the psychological laboratory was transferred to space on the first and fourth floors of the Anatomy Building, where it remained until 1901. In that year a two-story frame house at 5704 Ellis Avenue, located just north of where Ricketts North stands, was secured as the first exclusive home of what, in the following year, became the separate Department of Psychology. In this small edifice, supplemented by the Culver Hall basement room where Watson did his animal research work, all class, laboratory, research, and office work was carried on. Overcrowded from the beginning, this building was retained as the Department's animal laboratory for 21 years after 1908. In that year the other activities were moved to the more commodious quarters which the Department has occupied ever since, and Mr. Angell's description of "The New Psychology Building at the University of Chicago" in the November 1908 *Psychological Bulletin* makes entertaining reading to the present

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generation of students and visitors, whose admiration for these facilities seems more moderate than that formerly expressed. Nevertheless, later generations of students have agreed with a remark attributed to Mr. Angell, that "more psychology has been learned in the third-floor research rooms than in the first-floor lecture rooms."

One innovation was the provision of the Department's own machine shop in which much of the apparatus since used in Departmental research and instruction has been designed and constructed. In his 1902 report to the President (which, framed, hangs on the wall of the Departmental office), Mr. Angell stated a principle which has ever since been consistently adhered to: "In the securing of apparatus there has never been any purpose to create a museum in which specimens should be gathered for mere exhibit."

Another development was the Psychological Library. Psychological books and journals were originally housed in the general library in Cobb. In the 5704 Ellis building a few volumes were kept in the office, their number gradually increasing until a year or so after the Department had moved into its present quarters. Then the psychological journals were brought in and a more rapid growth began. The northeast second floor room became the Department library, one of a score or more on the quadrangles, with an overflow into the *Seminar Room* now occupied by the Department office. In the 1920's, this room, and later still another were added to the library, which by now was being increasingly used by readers from other Departments and schools, a reflection of psychology's widening affiliations and applications. Gradually the less used volumes were crowded off the shelves into unsuitable storerooms. Finally, in 1944, partly as a result of the trend toward centralization of libraries, partly due to the unsatisfactory, crowded condition of the collection and the constant fire-hazard, the staff reluctantly consented to the removal of the approximately 10,000 volumes to Harper, leaving a small reference collection in the laboratory for staff use, but freeing space for a more adequate Departmental office and seminar room.

A year after Mr. Lashley joined the Department in 1929 he was given for an animal laboratory the quarters in Culver Hall now occupied by Mr. Kluever. This continued to serve the Department until a year or two after Lashley's departure to Harvard in 1935. Later, the present quarters in Belfield were secured for Mr. Gulliksen's animal work. Meanwhile, Mr. Thurstone's psychometric laboratory had developed as a result of enlarged staff, improved equipment and widening areas of research. Increasing student enrollment in recent years has forced several Departmental classes into larger classrooms and labora-

tories than our building affords; so that the Department is now geographically quite dispersed.

Before the Department was separately organized, instruction in psychology was given by several members of the combined Department. The first year, psychological courses were offered by Charles A. Strong, Associate Professor of Psychology, who with several of his colleagues had come from Clark University to join the new University's staff, and by James Hayden Tufts, Assistant Professor of Philosophy, from the University of Michigan. That first year there was no full Professor or Head of the Department. In the first Register of the University we find Mr. Strong announced as offering an introductory course, using James' new *Briefer Course*, and a one-year advanced course providing "A critical study of James' Principles of Psychology with occasional lectures"; while Mr. Tufts offered an intermediate course using James' and Dewey's Psychologies, "with lectures and demonstrations." What was demonstrated is suggested by the course description in the following year, which reads: "Will be presented partly in the form of lectures, illustrated by experimental demonstrations; and each student will be given an opportunity to dissect an ox's eye and a sheep's brain." No one can doubt that psychology at the University of Chicago started off true to the Jamesian pattern.

## II. ESTABLISHMENT OF THE LABORATORY: THE ANGELL PERIOD

By the second year, the idea of psychology as an experimental science was so thoroughly accepted that experimental courses, a laboratory, and a new laboratory director were announced. This was, therefore, one of the earliest psychological laboratories established in an American University. The director, Assistant Professor James Rowland Angell, had been a young instructor at the University of Minnesota, with Bachelor's and Master's degrees from Michigan, and a Master's at Harvard under James, followed by a year at Berlin and Halle. He returned to take the Minnesota position without waiting to receive a Ph.D. degree, except for an honorary one many years later. (He also received 18 LL.D's.) His studies under Dewey at Michigan and James at Harvard equipped him to be the chief psychological spokesman for that distinguished group of philosophers, Dewey, Tufts, Mead, Moore, and Ames, who came to be known as the *Chicago School* of philosophical pragmatists and psychological functionalists. Angell was Director of the Psychological Laboratory from 1894 to 1902, and thereafter Head of the Department until 1920. For twelve years he was Senior Dean or Dean of the Faculties of Arts, Literature, and Science, and in 1918-19

acting President of the University. In 1920 he resigned to become President of Carnegie Corporation and a year later President of Yale. Since retirement in 1937 he has continued to be active as Educational Counselor for the National Broadcasting Company.

An interesting and effective teacher, who stimulated his students to independent and critical thinking, Angell could, at the same time, appreciate diverse and even conflicting viewpoints and give them a fair showing. Consequently, his introductory *Psychology*, published in 1904, which supplanted James' *Briefer Course* as the most widely used text in the new psychology, presents alternative views so objectively that on certain issues it is difficult or impossible to state with assurance his own position. Nevertheless, the text, like his lectures, presents clearly a *functionalist* point of view, with its stress upon mental activities as operations, not as contents; and with emphasis on their biological mechanisms, their social relationships, and practical utilities. This catholicity of view characterized not only the wide variety of courses he offered, but the research that came from the laboratory during his term as director and chairman. The 39 doctoral dissertations of the Department's first 18 years, as well as the four earlier theses mentioned before, represent a wide range of interests—sensory, perceptual, attention, thought, and imagery functions, motor learning, and the newer fields of animal, social, and differential psychology. It was characteristic of this atmosphere that doctors from this Department are to be found in almost every field and division of psychological work, and represent every viewpoint—except, perhaps, some of the more extreme ideologies.

Because of his growing preoccupation with administrative duties in the later years of his service, the actual direction of the laboratory devolved increasingly upon Mr. Carr, so that even in 1915 when I had my first contact with Angell as a teacher, his occasional visits to the laboratory were events rare enough to be remarked.

### III. TEACHING STAFF: THE DEWEY INFLUENCE

Not only from Angell, but from Dewey did the new psychological program get its direction. In 1894, two years after the opening, John Dewey came to Chicago from a professorship of Philosophy at Michigan, to become Head of the Department of Philosophy and Education, remaining for 10 years, when he went to Columbia University. The foremost figure in America, outside of James, in the new philosophical school of pragmatism (or, as he preferred to call it, *Instrumentalism*) he early became accustomed to being a storm-center of controversy, sometimes personal, oftener academic, but always a deliberate and dis-



passionate storm-center. While his influence on the psychology program operated principally through his associates and especially Angell, he did, throughout the ten years of his stay, offer a course in the Psychology of Ethics. This later found publication in the widely used Dewey and Tufts *Ethics*, and still later in his *Human Nature and Conduct*. His primary interest was in education and the development of a new-type laboratory school, described in his small but influential book *School and Society*. His views on the social goals and psychological methods of education have doubtless influenced American educational thought and practice more than those of any other educator.

During the first ten years, several other members of the combined Department, besides Dewey and Tufts, offered courses in psychology, among them George H. Mead, who taught comparative psychology, psychological methodology, and social psychology, Addison W. Moore, one of Angell's early assistants in the laboratory, Warner Fite who taught both laboratory and theoretical courses, and E. S. Ames, psychologist of religion. All these men except Fite remained for many years members of the Philosophy staff and their courses continued to attract some psychology students even after the Department's separation. Other names not so familiar to the present generation I shall not mention.

The second period, the 18 years of Mr. Angell's chairmanship, brought several staff members whose names are familiar to many of today's students. In 1903 John B. Watson received the first doctorate awarded in the Psychology Department. Following this he served on the staff for five years, going in 1908 to Hopkins as Professor of Experimental and Comparative Psychology. I have heard Mr. Carr say that Watson's introductory psychology class usually spent the first 9 or 10 weeks of the quarter on the nervous system. Even at that time he was enthusiastic over the idea of a psychology based on the reflex arc; although his first behavioristic pronouncement did not appear until 1912, after he had discovered Pavlov and the Conditioned Reflex.

The third doctorate in the Department was earned in 1905 by Harvey Carr who three years later returned as Assistant Professor and was identified with the Department until his retirement 30 years later. He told me that his first job was to supervise the remodeling of the new laboratory. Of his later work we shall speak presently. During this period, the Department's first teaching staff consisted of Angell and Watson, who were joined in 1907 for two years by Dr. Karl T. Waugh, and the following year by Carr. During this period, Professors Mead, Moore, and Ames of the Philosophy Department continued to contrib-

ute to Psychology programs, as did also Thomas of Sociology, Gore and later on Judd, Freeman, and Stevens of Education, and perhaps others who might be mentioned. Joseph W. Hayes, a 1911 Chicago Ph.D., now deceased, was instructor for several years in the second decade of the century, as were also Ellsworth Faris, later Professor of Sociology, and Harry D. Kitson, now of Teachers' College; while during the first World War years and immediately thereafter, the staff included Curt Rosenow, J. R. Kantor, now of Indiana, and R. T. Wiltbank, later at Knox College.

#### IV. THE CARR PERIOD

If we call the ten years in the Department of Philosophy the first period, and the years of Angell's chairmanship the second, the third period, that from 1920 to 1939, may be characterized as the period of Carr's leadership; even though for the first six years he was merely Director of the Laboratory, the chairmanship being held for four years by Charles H. Judd, Director of the School of Education, and for two years by James H. Tufts, Chairman of the Philosophy Department. Their influence, however, was almost altogether administrative, for only the former actually taught a course in the Department. While Carr's well known interests, in space perception, memory, and animal and human learning, are apparent in many of the doctoral theses of that period, several were written under the supervision of other men in the Department, as well as a few in other Departments, notably Education and Physiology. Later, after Professors Thurstone and Lashley joined the staff, psychometric and psychobiological studies became numerous and important. Their own contributions are too well known to call for description or evaluation here. In 1926 Carr was appointed Chairman of the Department, a position he retained until his retirement in 1938. In 1925 appeared his *Psychology*, perhaps the clearest presentation we have of the later, more biological formulation of a functionalist point of view. Carr himself, however, has always protested against being tagged with any particular label, even *functionalist*, holding that the views he stands for have too much in common with those held by others to justify thus setting them into contrast. This distaste for stereotyped classifications and rules characterized also his attitude of encouraging independence and self-reliance in his students. His scrutiny and criticism of research proposals and reports was always searching and detailed, but he was not dictatorial or dogmatic in his criticisms. He would ask, "What do you propose to do about this difficulty?" or "Have you thought of that possibility?", while appeals for advice were charac-

teristically met by the reply, "It's up to you; do what *you* think best." It is not surprising that more than a hundred doctors and perhaps as many masters, whose dissertations he supervised or criticized, recall him with admiration and affection as a teacher who did his best to make them independent and resourceful scholars, not disciples.

This period saw the coming and going of a good many young teachers of introductory sections, most of them with fellow's or assistant's rank, and most of them later taking their doctorates. Up to a few years ago it was believed that introductory psychology could not properly be taught except in small sections; and the post-war years had brought a large influx of undergraduates. Some of these names would be unfamiliar to most readers, although some will be recognized as having later achieved some eminence in their fields, such as Blatz, Heron, Darrow, Sheldon, and others perhaps as well known as these. Other staff members of higher rank handled advanced courses and supervised research, among them the late Edward S. Robinson, later of Yale, Florence Richardson (later Mrs. Robinson) who carried on the psychological work in the School of Business, Arthur Kornhauser also of the School of Business, Arthur G. Bills, now of Cincinnati, Capt. H. A. Swenson, for long advisor in the College and instructor of introductory courses, K. S. Lashley, L. L. Thurstone, M. W. Richardson, H. Kluever, Harold Gulliksen, and the writer. All of these I have named (except Lashley and Kluever) were doctors of this Department, which fact occasionally used to lead to comments within the Departmental staff about the "dangers of academic inbreeding"; although, knowing the diversity of views and interests among those trained in the Department, I have never worried much about this hazard.

The account of this third period would be incomplete without mention of other men who have contributed in no small measure to its work—Dr. H. H. Donaldson of Anatomy, and later Dr. C. Judson Herrick, whose course in Neurology has for years been a higher degree requirement, and an opportunity for philosophical speculation as well; and now Drs. Bartelmez and Polyak. Within the laboratory, generations of students have come to know Dr. K. S. Yum, himself a doctor in the Department, who for so many years was Departmental Librarian. Our friends in related Departments who have contributed to the Psychology program are too many to begin to enumerate. Members of this Department have always cooperated with others on border-line or joint-interest problems, and have shared their field of work with psychologists in other Departments. From the first, research and instruction in educational psychology has been the responsibility of the Department

of Education, but psychologists in both groups have always been on friendly and cooperative terms, and students have moved back and forth freely in working out their programs. The same kind of relations have been maintained with psychologists in the School of Business, Home Economics (child psychology), Medicine and Psychiatry, and other Schools and Departments having psychological interests; and the work of all these psychologists well deserves more mention than this brief account of the Department permits. In addition, public and private institutions, in city and state, have generously provided facilities for demonstration, research, and apprentice-training without which the Department's work would have been far less effective; we may cite, as representative, the Institute for Juvenile Research, the Chicago Public Schools, and several state hospitals. Psychologists from other Universities have also made their contribution to the Department's program. It has always been the practice to invite visiting professors of reputation, usually during summer quarters. Ladd of Yale and Baldwin of Princeton were early guest-professors. I recall also the names of others, including Scott, Pillsbury, Watson, Sutherland, Joseph Peterson, Faris, June Downey, Yerkes, Warner Brown, Franz, Griffith, Hunter, Stratton, Angier, Bott, Perrin, Rosenow, Herman Adler, Hull, Taylor, K. Buehler, Culler, Paterson, J. J. B. Morgan, Darrow, B. F. Skinner, Grace Munson, and Farnsworth, besides Thurstone, Lashley, and Rogers who later joined the staff, and perhaps others. All of these, therefore, the Department can claim as past members.

#### V. THE RECENT YEARS

When Mr. Carr retired in 1938, no provision had been made for a successor. Departmental relations with the Biological Sciences Division were a bit disturbed, and for a time the status of the Department was somewhat confused. During the summer and autumn of that year Mr. Thurstone stepped into the breach, and brought into the Department as full time members two men who had been assistant examiners with him while he was University Examiner, and had been serving part time as instructors in the Department, Messrs. Gulliksen and Wolfe; a third, M. W. (now Lt. Col.) Richardson, went to Washington as Chief Examiner in the Civil Service. That winter the Department's administrative affiliation was transferred to the Division of the Social Sciences, retaining, however, representation in the Biological Sciences, and continuing to award the B.S. and Divisional M.S. degrees in that Division. With the transfer came a new Chairman, an educational psychologist of note, Frank N. Freeman, of the Department of Education. At his

request the writer assumed the title and duties of Secretary of the Department. Unhappily, Mr. Freeman's chairmanship was very temporary, since he resigned that summer to accept appointment as Dean of the School of Education of the University of California, where he is still active. The Department's administrative responsibilities thereupon devolved upon the Secretary, where they have remained ever since, although the title was superseded in 1945 by that of Acting Chairman.

The war, as we all know, has severely disrupted the Department's work in many ways, not least by the absence for war service of several staff members. These gaps, temporarily filled by J. E. P. Libby, L. R. Tucker, Edith Sherman Jay, Virginia Brown and others, are now provided for on a more enduring basis by the appointment of A. W. Brown, for long a part-time lecturer in the Department, and T. G. Andrews, and for the coming year, that of C. R. Rogers and the expected return of Dr. Wolfe. We look forward hopefully to the appointment of additional staff members, to provide the means of accomplishing our Departmental goal of contributing substantially to the growth and usefulness of psychological science.\*

#### VI. DOCTORATES IN THE DEPARTMENT

But the most important thing in the Department's history is not, of course, its housing and equipment, nor its teaching staff, but its research and training output, including hundreds of graduates who have gone out to continue their careers of productivity and service, and especially those who have received its highest recognition, the Doctorate of Philosophy.

In the 42 years of the Department's independent existence, 160 men and women have earned its Ph.D. degree, 115 men and 45 women. Of these, 39 received the degree during the period of Mr. Angell's chairmanship, 1903 to 1919; 108 between then and 1938, the year of Mr. Carr's retirement; and 13 since that date. Numerically, the high points were 1931 with 12, 1928 and 1930 with 11 each, and 1917 with 10; the low points, years in which no doctorates were awarded, were 1906,

\* *Postscript, May, 1946.* Some changes since this paper was prepared a year ago may be noted. Dr. Wolfe's resignation to become Executive Secretary of the American Psychological Association was followed by the appointment of William D. Neff as Assistant Professor, and of Virginia Brown, Virginia Axline, and M. H. Groves as Instructors for 1946-47; while negotiations are under way looking to three or more new appointments at professorial levels. The current influx of graduate students, now nearly four times as many as a year ago, has brought the student-load to its peak and increased instructional, staff, and space problems, so that limitation of admission now seems inevitable.



1918, and 1919 (during the first World War), 1923, and 1944 (the second World War). Even before 1942 Chicago doctors in Psychology were scattered all over the world, including every part of the United States and Canada. To read the titles of the dissertations is of interest, for they give a clear picture of the trend of interests among staff and students of the Department. Any perfect or even reliable classification of these topics is, of course, impossible; but even such a rough classification as the following, which is based primarily on subject-matter, is, I believe, illuminating. The numbers given are percentages of the total output of 160:

<i>Learning</i> , of which 19% were verbal learning, and 13% motor learning	32%
<i>Other motor functions</i> , including emotion and suggestion	9%
<i>Psychobiology</i> , including effects of drugs, diet, vitamin deficiency	9%
Beginning in 1917 with Arlitt's studies of the behavior of alcoholic rats, but mainly done under Lashley	
<i>Individual differences and their measurement</i>	9%
Beginning in 1915 with Kitson's study of college students	
<i>Psychophysics and psychometrics</i>	8%
Plus some classified under other headings	
<i>Personality and social psychology</i>	8%
Throughout the history, beginning with Faris's study on punishment in 1914	
<i>Sensory functions</i>	8%
The majority before 1917	
<i>Perception</i>	6%
Most before 1930	
<i>Other processes—imagery, thought, attention, affection, aesthetics</i>	6%
Most earlier than 1920	
<i>Animal psychology</i> , undifferentiated topics	2%
All before 1920	
<i>Pathological behavior</i>	2%
<i>Child and genetic</i>	1%
	<hr/> 100%

Master's degrees have during this time been awarded to 116 students, 22 of whom later earned their doctorates in the Department and several more in other Departments of the University or elsewhere. I have not attempted to classify their dissertations, but I am sure, from casual inspection, that it does not differ widely from the one just described. We have no count of Bachelor's degrees in Psychology prior to 1932, when the Divisional plan with provision for Departmental programs began; but 268 Bachelors of Arts and Bachelors of Science in Psychology, not quite 21 per year, have been awarded since then.

Until comparatively recent years, most doctoral dissertations (as

well as some Masters' and other laboratory studies) found publication in the *Psychological Review Monograph Supplement* (later, *Psychological Monographs*), which Mr. Angell edited for some years. Most of these were parts of a Chicago Laboratory Series. Other studies, especially animal, were published in the *Journal of Comparative Neurology and Psychology*, the *Journal of Comparative Psychology*, and the *Behavior Monographs* (Chicago Series). During the 1920's, the University for some years required publication only of abstracts of theses, in its own series of annual volumes, although during this period some were published in full elsewhere. Since then, as dissertations in psychology have tended to become more concise and less discursive, most have been published in full or in "essential" form, many of them in the *Journal of Experimental Psychology* and *Psychometrika*. But there is hardly a psychological journal in the English language which has not contained one or more studies from the laboratory; while throughout the Department's history, its staff members have served as editors or on the editorial boards of many psychological journals.

This list of 160 doctors includes many names familiar not only to psychologists and educators, but, in some cases, to a much wider public. Most of those later associated with the Department as staff members have been mentioned; while others of the earlier doctors who have achieved recognition through research, publication, administration, or teaching, include many of whom we can name only a few. The first period (Philosophy) includes, among other psychologists, E. S. Ames, D. P. Macmillan (long in the Chicago Schools), Helen Thompson Woolley, and Kate Gordon Moore. The Department's own roster includes such names as Beardsley Rumr, Chi Weh Luh of retention curve fame (the first of 13 Chinese graduates), June Downey, Walter Van Dyke Bingham, Joseph Peterson, C. S. Yoakum, Walter S. Hunter, L. A. Pechstein, Ada H. Arlitt, Margaret Wooster Curti, Helen Koch, E. A. Culler, Dr. William E. Blatz, Thelma Gwinn Thurstone, Chester Darrow, Chaio Tsai, now one of the world's foremost physiologists, the late John McGeoch, Dr. W. H. Sheldon of Somatotype fame, President Goodrich White of Emory University, Dr. Mandel Sherman, Kate Hevner Mueller, Dean of Women at Indiana, former President W. H. Cowley of Hamilton, Marion Monroe, the reading specialist, and dozens of others not less worthy of being named. Quite deliberately I have omitted mention of the 55 holders of Ph.D.'s granted since 1930, for it is still a bit early to appraise fairly their achievements.

Of the 160 doctors, over a hundred have pursued their careers in college and university positions. More than a score of these are or

have been heads of University Departments, and a dozen have held major administrative appointments, deanships or presidencies. A cross-section of the psychological and other vocations they represent would probably resemble a fair sample of the membership of the American Psychological Association. Besides psychological teaching and research, Chicago doctors are to be found in many branches of public service, national, state, and municipal (not to mention army, navy, and civilian war work), in educational administration, notably as school psychologists, in industry and business, in counseling and guidance, and in public welfare institutions of many kinds; while at least seven hold medical degrees. I estimate that well over two-thirds of the 160 have actively continued since their doctorates to produce scholarly research and publication, while still others have contributed to the literature of educational, business, or public welfare administration.

Besides our own doctors, seven National Research Council Fellows—C. F. Jacobsen, I. Krechevsky, R. W. Leeper, N. R. F. Maier, L. A. Pennington, T. C. Schneirla, and P. H. Settlege and one General Education Board Fellow, W. A. Varvel, have carried on post-doctoral studies with members of the Department staff, most of them with Lashley; while through the years, a large number of other visiting doctors have worked in the Department's laboratories for longer or shorter periods.

#### VII. THE DEPARTMENT AND PSYCHOLOGICAL ORGANIZATIONS

It is of interest to note that the Psychological Journal Club is even older than the Department. A Philosophy Club first announced in the 1894-95 Register was supplemented in 1901-02 by a Psychological Journal Club, meeting bi-weekly, its principal work being to present critical reports upon current experimental literature. This function continued to characterize the club's activities until about 15 to 18 years ago—I do not recall the exact date. At that time, long-continued efforts to bring undergraduates and first-year graduate students into more active participation led to the formation of a Junior Psychological Club, whose weekly meetings featured an address, often by a visiting lecturer from some other institution or Department. Meanwhile, the advanced students continued the Senior Journal Club along established lines. But the Junior Club became so popular that it attracted the members of the older club, and after a year or two the Senior Club was discontinued, and the Psychological Club continued to function after the new pattern. Most of us have felt that the change brought a real loss to the Department's training function; and while current dissertations and other researches have been presented, only within the last

year or two has the earlier "Journal" function been revived. Many of us hope that this valuable opportunity for public appearances by embryo psychologists may resume something of the importance it once had.

Psi Chi, the honorary psychological society founded in 1929, has from the start had a chapter here, but it has seldom played the important role it has in many institutions, because of the older and stronger tradition of the Journal Club. However, the current movement to revive and increase its functions and membership promises well for its continuing usefulness at Chicago. Meanwhile, the national organization has never been without important Chicago contacts; the Department's alumni have furnished two national presidents, McKinney and Fritz, each serving two years, and two national secretaries, Louise Grossnickle Yum and Dorothea Ewers. Another national honorary organization, the scientific society of Sigma Xi, has over many years elected to its membership a good many prospective doctors in Psychology; while the Department's bachelors have always been represented among those elected to Phi Beta Kappa.

The Department has also had recognition by national and regional associations. The American Psychological Association has on five occasions been the guest of the Department, in 1901, 1907, 1915, 1920, and 1933; and nine of its past presidents, the highest honor bestowed by American psychologists, have been Chicago doctors or staff members—Dewey, Angell, Judd, Watson, Carr, Lashley, Hunter, Thurstone, and Peterson, as well as several others who have had summer-quarter visiting appointments in the Department. The Midwestern Psychological Association has met here twice since its establishment in 1926, and its presidents have included five Chicago alumni, Thurstone, McGeoch, Carr, Bills, and Culler. So the Department has played its role not only locally but regionally and nationally.

Members and alumni of the Department have reason to look back with something of pride upon its record of accomplishment. But a worthy past is not enough. We look forward with hope and confidence that the years to come will bring a still more noteworthy record of Departmental contribution to the growth and usefulness of our science.

## NEW STATISTICAL CRITERIA FOR LEARNING AND PROBLEM SOLUTION IN EXPERIMENTS INVOLVING REPEATED TRIALS

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Psychologists generally have barely begun to utilize the variety of statistical techniques available to demonstrate learning and problem solution in experiments involving repeated trials. Many studies still appear using arbitrary criteria unsupported by probability statements, in spite of their availability and desirability. Also some methods have become available only recently and for that reason have not been used. The present paper includes a brief discussion of previously used procedures and presents two new criteria which have not been used in psychological research, but which seem to offer possibilities for more complete analyses of learning, problem solution, and other forms of behavior which have been studied by means of repeated trials. Some of the types of experiments to which these techniques are applicable include simple and complex discrimination experiments, variations of the Hamilton "perseverance" problem, food and other preference tests, spatial and mental mazes, string-pulling tests, Weigl and other sorting tests, delayed reaction experiments, conflict experiments and simple social reaction studies.

### PREVIOUSLY USED CRITERIA

All experiments involving repeated trials on the same basic task give rise to problems of assessing degree of improvement. In most instances the subject may "solve" the problem by chance, and a certain proportion of his correct responses are therefore suspect. The exact number of chance solutions cannot, however, be determined, and the experimenter must use some indirect procedure to decide whether or not his subject's performance is other than what might be "expected" by chance. There have been two frequently used ways of determining whether a problem has been learned or solved. A non-statistical arbitrary level of performance has been the most widely used criterion even in the recent past. For example, a rat is required to run the maze with no errors one or more times (8), or the subject must select the correct one of two stimulus objects 18 out of 20 times, getting the last ten correct (9). The other frequently used criterion is set up in terms of the probability of the subject's making a certain *total number of correct responses* by chance (5, 7, 16). With this method, the experimenter as-



sumes that the subject has learned the task if the probability of his responding correctly by chance as many times as he did actually respond falls below a conventional (arbitrary) level.

[illegible]

The new techniques to be described in the present paper are statistical tests which should permit the experimenter to demonstrate "what is happening" more completely than he has heretofore been able to do.

## INTERPRETATION OF STATISTICAL CRITERIA

The reasoning behind the statistical tests to be described follows a familiar pattern. Consider an experiment set up so that the subject is given  $n$  trials with a constant probability,\*  $p$ , of responding correctly. Krechevsky's "Hypotheses" experiment (7) serves as a good example. In it, the night's work for each subject consisted of solving 40 discriminations—i.e.,  $n=40$ —each of which involved a choice of one of *two* brightnesses—thus  $p=.5$ . To demonstrate that the subject has learned or solved the problem, it is first necessary to hypothesize that he is responding at random with respect to the critical variable:

\* Using the classical definition, the probability of the occurrence of a correct response is obtained by enumerating the total number of equally likely, mutually exclusive possible responses under the specified conditions and dividing this number into the number of equally likely, mutually exclusive responses designated as correct.

i.e., that he has not learned.† (Next is deduced mathematically the probability of a chance performance equal to that of the subject) Sometimes this is done directly, but more often it is convenient to compute the expected (average) random performance and some measure of series to series variability from the expected value. Then the subject's actual performance is considered as a chance deviation from the expected value in terms of the measure of variability, and the mathematical probability of such a chance deviation is assessed. Then, finally, if the probability of a chance deviation as great or greater than that represented by the actual performance falls below a certain level the experimenter will reject the hypothesis that the behavior is random with respect to the critical variable. This constitutes the statistical test. The experimenter then assumes that inasmuch as the critical variable was randomized with respect to the other variables his subject has learned or solved the problem.

#### THE CRITERION OF TOTAL NUMBER OR PROPORTION OF CORRECT RESPONSES

Before considering new criteria, the commonly used statistical criterion of total number or proportion of correct responses will be discussed. The mathematics of this test involves determining the probability that the actual number of correct responses equals or exceeds a given number,  $m$ , in a sequence of  $n$  independent trials where the probability of a chance success on a single trial is  $p$ . This involves the Bernoulli distribution, which is dealt with by most books on mathematical probability or statistics (1, p. 197; 13, p. 44). The exact probability of  $m$  correct responses can be shown to be:

$$P_m = C_m^n p^m q^{n-m}, \quad [1]$$

where  $C_m^n = \frac{n!}{m!(n-m)!}$ ,  $n!$ , read  $n$ -factorial, equals  $1 \cdot 2 \cdot 3 \cdots n$ , and

$q = 1 - p$ . The experimenter is interested in the total probability that his subject might by chance do as well or better than  $m$  successes, and this quantity will, of course, be equal to the sum of the probabilities of  $m, m+1, m+2, m+3, \cdots, n$  correct responses. Written as a formula,

† Note that this does not necessarily assume that the subject is responding entirely at random within the experimental situation. He may be responding by position, by simple alternation of position, or show some other "hypothesis" behavior, but the reasoning does assume that the critical variable, whatever it is, has been varied randomly with respect to all other discriminable stimuli.

the total probability that the number of correct responses equals or exceeds  $m$  is:

$$P(m) = \sum_{i=m}^n C_i^n p^i q^{n-i} \quad [2]$$

As a concrete example of the use of [2],† suppose that a rat responds correctly in 19 out of 25 trials in an experiment such as that of Krechevsky, mentioned above. In this instance  $p = .5$ ,  $n = 25$ , and it is necessary to find the total probability,  $P$ , that the performance equals or exceeds an  $m$  of 19. Using [2],  $P(m \geq 19)$  will be:

$$\begin{aligned} & \frac{25!}{19!6!} \left(\frac{1}{2}\right)^{25} + \frac{25!}{20!5!} \left(\frac{1}{2}\right)^{25} + \frac{25!}{21!4!} \left(\frac{1}{2}\right)^{25} + \frac{25!}{22!3!} \left(\frac{1}{2}\right)^{25} \\ & + \frac{25!}{23!2!} \left(\frac{1}{2}\right)^{25} + \frac{25!}{24!1!} \left(\frac{1}{2}\right)^{25} + \frac{25!}{25!0!} \left(\frac{1}{2}\right)^{25} .^{**} \end{aligned}$$

Which, accurate to eight places (in order to use all terms for illustrative purposes), is:  $.00527799 + .00158339 + .000377001 + .00006855 + .00000894 + .00000075 + .00000003 = .00731665$ . This would be rounded off to .0073. It means that the probability of the subject's getting 19 or more correct by chance out of 25 trials would be .0073, or that about 73 times in ten thousand such 25-trial sequences would the subject be expected to do as well as or better than it actually did if it were responding at random with respect to the critical variable. In this instance the experimenter could reject the hypothesis of random responses at the 1% confidence level.

Formula [2] is not particularly complicated and should be used wherever applicable, but in spite of this, most such total probabilities have in the past been determined by means of the normal curve which gives a rough approximation to the true value of  $P(m)$ . To enter the table of the normal curve, the normal deviate,  $x/\sigma$ , must be computed (cf. 4, Table 15, p. 110). In this fraction  $x = m - np$ , and  $\sigma = \sqrt{npq}$ ; thus:

$$\frac{x}{\sigma} = \frac{m - np}{\sqrt{npq}} \quad [3]$$

\* This is the familiar formula for Total Probability (1, 13). The index,  $i$ , in this notation shows the part of the formula which varies in forming each successive term in the sum. The limits of summation below and above the summation sign indicate that in the first term  $i = m$ , in the next term  $i = m + 1$ , etc., until finally  $i$  equals  $n$  in the last term (cf., 15, Ch. 6).

† Square brackets will be used in the present paper in referring to the various formulae.

\*\*  $0! = 1$ .

To find the approximate probability of 19 or more correct out of 25 trials, as in the example above, using formula [3], and the table of the

normal curve,  $\frac{x}{\sigma} = \frac{19-12.5}{\sqrt{25 \cdot (.5)(.5)}}$ , a normal deviate of 2.6. This would

be interpreted from a table of the normal curve to give a probability of about .0047. The very considerable error of the approximation is thus .0047 - .0073, or -.0026.

The probability obtained from [2] may also be approximated by use of the  $\chi^2$  distribution for one *degree of freedom*. With this method the number of correct responses may be designated  $m$ , and the number of incorrect responses may be designated  $s$ ; then  $\chi^2$  is given by the equation (2, p. 334):

$$\chi^2 = \frac{\left(m - \frac{p}{q} s\right)^2}{\frac{p}{q} n}, \quad [4]$$

which for  $p=q=.5$  reduces to:

$$\chi^2 = \frac{(m - s)^2}{n} = \frac{(m - s)^2}{m + s}. \quad [5]$$

In the above example  $\chi^2 = \frac{13^2}{25} = 6.76$ . The probability is found from

tables of  $\chi^2$  to be less than .01 but greater than .005 (3, 12).

In general, equation [2] should be used to compute the probabilities for the Bernoulli distribution, but the normal curve and  $\chi^2$  may be used under certain conditions. The limiting conditions for the use of [3], [4], and [5] are as follows: for [3] the normal curve gives very inaccurate approximations if  $p$  or  $q$  are nearly unity or if  $n$  is not fairly large ( $n > 30$ ); for use of [4] and [5] both  $np$  and  $nq$  must exceed five, which means that  $n$  must in all cases exceed 10, and if  $p$  and  $q$  do not equal .5,  $n$  must be greater than 10. In general [4] and [5] will give more accurate approximations than [3] when  $n$  is small.

#### RUNS AS A CRITERION OF LEARNING AND PROBLEM SOLUTION

The responses of a successful subject are characterized by features other than a high *proportion* of correct responses, and some of these

other features may be used as criteria of solution. Suppose that a subject responds randomly to the two-choice discrimination problem for the first fifteen trials, and then responds correctly for the last ten trials. The total correct criterion would fail to demonstrate learning for this subject. It is, however, possible to determine the total probability of at least one "run" of length  $s$  or greater in a sequence of  $n$  trials with a constant probability of  $p$  of a chance success on any one trial. The probability of a run of ten in a series of 25 with a single-trial probability of .5, as in the data above, would be .0083, and an experimenter, if he accepted data at the conventional 1% confidence level as significant, would assume that his subject had solved the problem.

In general, the probability that there will be one or more runs of  $s$  or more chance successes in  $n$  trials where the probability of a chance success on each trial is  $p$  will be:  $P(s) =$

$$1 - \sum_{i=0}^{n/(s+1)} (-1)^i C_i^{n-is} (qp^s)^i + p^s \left[ \sum_{j=0}^{(n-s)/(s+1)} (-1)^j C_j^{n-s-(j+1)} (qp^s)^j \right]. \quad [6]$$

Equation [6] is based upon the solution given by Uspensky (13, p. 74) As an example of computation, using [6], consider the probability of one or more runs of ten or more in a series of 25 trials as in the paragraph above. In this case [6] becomes:

$$\begin{aligned} 1 - \sum_{i=0}^2 (-1)^i C_i^{25-10i} \left( \frac{1}{2} \right)^{11i} + \left( \frac{1}{2} \right)^{10} \left[ \sum_{j=0}^1 (-1)^j C_j^{25-10-(j+1)} \left( \frac{1}{2} \right)^{11j} \right] \\ = 1 - 1 + C_1^{15} \left( \frac{1}{2} \right)^{11} - C_2^{15} \left( \frac{1}{2} \right)^{22} + \left( \frac{1}{2} \right)^{10} \left[ 1 - C_1^5 \left( \frac{1}{2} \right)^{11} \right] \\ = 1 - 1 + \frac{15}{2048} - \frac{10}{(2048)^2} + \frac{1}{1024} \left( 1 - \frac{5}{2048} \right) = .0083. \end{aligned}$$

Computations with [6] are fairly elementary but rather tedious, and therefore the probabilities for various length runs with  $p = .5$  are given for convenient  $n$ 's in Table 1. The use of Table 1 is extremely simple. By selecting the column corresponding to the number of trials in the sequence and following it down, one can find the probability of a run of any length from six up to runs which are significant at the 0.1% confidence level. Evaluation of probabilities from the table may be illustrated with some unpublished discrimination data obtained by Harlow in the Wisconsin Primate Laboratory from a monkey solving a Spence-type (9) problem.\* Correct choices are indicated by R,

\* In this problem the subject had to select a stimulus object which has been incorrect in previous discriminations.



and incorrect choices are indicated by *W*. The data are as follows: *WRRWWRRWWRRRRRRRRRRWR*. The longest run is nine correct choices in the series of 20 trials. The probability of a run of nine in 20 trials is given in Table 1 as .0127, which means that such data would be expected by chance less than twice in a hundred such 20-trial sequences. Note that the data do not approach significance in terms of the total correct criterion.

TABLE I

PROBABILITIES OF ONE OR MORE RUNS OF **S** OR MORE CHANCE SUCCESSES IN A  
SERIES OF **N** TRIALS WITH A CONSTANT SINGLE-TRIAL  
PROBABILITY OF 0.50

S \ N	16	20	25	50
6	.0929	.1223	.1577	.3146
7	.0429	.0582	.0770	.1653
8	.0195	.0273	.0369	.0836
9	.0088	.0127	.0176	.0415
10	.0039	.0059	.0083	.0204
11	.0017	.0027	.0039	.0100 +*
12	.0007	.0012	.0018	.0049
13	—	.0006	.0009	.0024
14	—	—	—	.0018
15	—	—	—	.0008

\* This value is slightly over .0100, so that a run of 12 would, therefore, be considered significant at the 1% confidence level.

The criterion of runs will prove most useful in instances where the mode of response is likely to shift suddenly—so-called insightful or hypothesis behavior—and when little oscillation (6) may be expected to occur. In data where oscillation is appreciable, the total correct or some other criterion should be used. A variation of the runs criterion may prove to be very useful in cases of partial learning where there is considerable oscillation and also in analytic study of the learning process. This variation is described below.

## THE GROUPING OF SUCCESSES AND FAILURES AS A CRITERION OF LEARNING AND PROBLEM SOLUTION

During a sequence of random responses, the subject tends to shift frequently from one response to another in the same fashion that heads and tails alternate frequently in a random series of coin tosses. An example of such a random series of ten coin tosses is:

*T, H, T, T, H, T, H, H, H, T.*

The series consists of four groups of one or more *tails*, and three groups of one or more *heads*, making a total of seven groups. With five heads and five tails—as there are here—the maximum possible number of groups would be ten, and the minimum possible would be two. In general, the greater the systematization or non-randomness of a behavior sequence, the fewer\* will be the number of groups. A test of randomness may, therefore, be made in terms of the probability of the actual number of groups,  $u$ , in the data being less than or equal to any given number  $u'$ .

If  $m$  is the number of responses of one sort and  $n$  is the number of responses of the other sort ( $m \leq n$ ), then Stevens (10) and others (14) have found the probability that  $u$  is less than or equal to  $u'$  to be:

$$P(u \leq u') = \frac{m!n!}{(m+n)!} \sum_{u=2}^{u'} f_u, \quad [7]$$

where

$$f_u = 2C_{k-1}^{m-1}C_{k-1}^{n-1}, \text{ if } u = 2k \text{ (i.e., } u \text{ is even),}$$

and

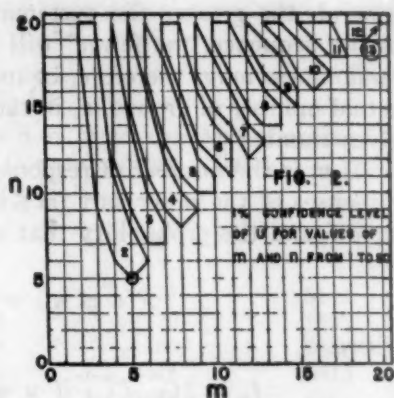
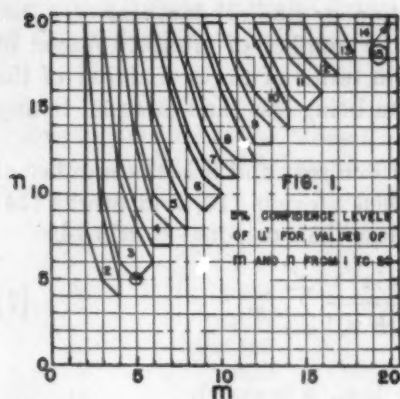
$$f_u = C_{k-1}^{m-1}C_{k-2}^{n-1} + C_{k-2}^{m-1}C_{k-1}^{n-1}, \text{ if } u = 2k - 1, \text{ (i.e., } u \text{ is odd).}$$

Tables of this probability function have been published by Swed and Eisenhart (11). For convenience the 5% and 1% confidence levels of  $u'$  have been graphed from Swed and Eisenhart's Table II in Figs. 1 and 2, respectively, for values of  $m$  and  $n$  up to 20.

Use of Figs. 1 and 2 may be illustrated from the coin tossing example given above. Here  $m$  and  $n$  both equal 5, and this point has been encircled in each figure. In Fig. 1 the encircled point lies in the area for  $u' = 3$ . Hence it may be concluded that the example, in which there are seven groups; i.e.,  $u = 7$ , does not depart significantly from chance grouping. (In Fig. 2 the point corresponding to  $m = n = 5$  lies in the area for  $u' = 2$ . Significance at the 1% level, of course, requires an even smaller number of groups than significance at the 5% level.) As another example of the use of Figs. 1 and 2, consider the following raw data from a "problem-finding" study on the rhesus monkey (17): *RRWWWWWWRRRRRRWRRRRRRWWRR*. Here there are ten wrong responses and 15 right responses, forming *seven* groups. From Fig. 2 it may be seen that when  $m$  is ten, and  $n$  is fifteen, seven groups

\* Systematization may also be shown by an unusually large number of groups. This will not be as generally useful to psychologists as the test described. Consult original references.

will be significant at the 1% level, hence the above series might be said to show systematization or partial learning. Note that neither the total correct nor the runs criterion would show the significant departure from a random sequence.



FIGURES 1 AND 2. THE 5% AND 1% CONFIDENCE LEVELS FOR  $u'$ , PLOTTED FOR VALUES OF  $m$  AND  $n$  FROM ONE TO 20.

In using these Figures, the point determined by the values of  $m$  and  $n$  ( $m \leq n$ ) is located in each Figure. This point will, in each Figure, lie in one of the irregularly shaped numbered areas. The number of the area is the 5% or 1% value of  $u'$  in Figures 1 and 2, respectively.

The grouping test differs from the other tests discussed in the present paper in that no defined  $p$ , or single-trial probability need be specified. Thus non-random grouping of wins or losses in an experiment on competition could be demonstrated, or a sequence of social contacts between nursery school children might be investigated for grouping of approach or avoidance responses, or the positive and negative discrepancy scores of a single subject in a level of aspiration experiment might be analyzed. This generality makes the grouping test far-reaching in its applications. The only assumption involved is that the variable behind the measures is continuous in the general population, and the null hypothesis is that the items are selected independently at random; *i.e.*, all grouping is random.

The grouping criterion is usually most sensitive when  $m$  and  $n$  are about equal—a situation when the other criteria usually fail to demonstrate non-random sequences. It is therefore likely that this criterion may be of especial use in the stages of learning and problem solution

when partial and temporary solutions introduce the first systematization into the behavior sequences.

Another instance in which the grouping (and also the runs) criterion are especially useful would be in making tests of incorrect modes of solution such as Krechevsky made (7), using the total correct criterion. Some subjects; *e.g.*, human adults, shift away from incorrect modes of response so quickly that it is difficult to "catch" them with a statistical test. The abrupt shifts however, are advantageous with the *grouping* criterion and the *runs* criterion. It is not feasible to list all possible uses to which these tests may be put, but it is hoped that there will be a large number of experimental situations in which these new tools will be found to be helpful. Future research and analysis in the various areas in psychology will show the extent of their usefulness, and the interpretations to be attached to results obtained by their application.

#### SUMMARY

The logic of statistical tests of learning and problem solution in repeated trials was outlined. Statistical tests already in use based upon *total correct responses* in a sequence and new statistical tests in terms of (a) *length of runs of correct (or incorrect) responses*, and (b) *grouping of successes and failures* were described and illustrative examples were given along with tables and figures to facilitate the use of the new tests. Suggestions were given as to possible applications of the new tests.

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## BOOK REVIEWS

DEJONG, H. H. *Experimental catatonia*. Baltimore: Williams and Wilkins, 1945. Pp. xiv+225.

DeJong defines and describes human catatonia in terms of the hypokinetic and hyperkinetic psycho-motor symptoms. Among the hypokinetic phenomena are diminished motor activity, catalepsy, and negativism. Among the hyperkinetic phenomena are impulsiveness, aggressiveness, and symptoms which depend upon activity of the autonomic nervous system, such as excessive salivation and blood volume changes. In experimental catatonia in animals the hypokinetic phenomena are shown by diminished motor activity, catalepsy, and physiological negativism. The hyperkinetic phenomena are shown by running, jumping, and by autonomic nervous system symptoms.

Having thus defined experimental catatonia and its relation to the catatonia seen in psychiatric patients, DeJong describes and gives protocols for a large number of experiments done by himself and his collaborators in the production of experimental catatonia in a variety of animal forms. His original experiments were done with bulbo-capnine. The injection of this drug brings about a decreased motor activity, negativism, catalepsy, and salivation, which seem quite similar to human catatonia. He demonstrated that the action of the drug is directly upon the central nervous system. A series of experiments indicated that the drug did not act on any particular portion of the central nervous system, since it was impossible to interfere with the catatonic symptoms by the ablation of any particular part of the brain.

DeJong worked through a long series of other drugs, trying to find which ones would bring about the catatonic symptoms as well as the determination of the particular molecular form or portion of the chemical which might be active. He found a long list of chemical substances which would bring about catatonic symptoms, but he was not able to solve the problem of whether or not there existed a chemical "catatonizing nucleus."

The work with the chemicals indicated that symptoms were due to an interference with the oxygen supply or oxygen utilization by the brain. On the basis of this hypothesis he conducted a series of experiments showing that partial asphyxiation brought about in a variety of ways did produce the catatonic syndrome. Whether or not this was due to oxygen lack was not finally demonstrated since asphyxiation is accompanied by failure in other nutritional supplies and by a change in the acid-base ratio of body fluids.

Another series of experiments was carried out attempting to produce catatonic symptoms by neuro-surgical interference with various brain structures. No regular or conclusive results were obtained. He found

that rats placed in a centrifuge and spun rapidly showed catatonic symptoms. In all probability the symptoms were due to brain injury and to the interference with brain metabolism.

He next reports a series of experiments carried out with rats, in which he produced catatonic symptoms by electrical stimulation and by loud, continuous sound stimuli. He was unable to produce catatonic symptoms in humans when similar stimulating conditions were employed. A series of experiments were conducted in which catatonic phenomena were produced in dogs and cats by alterations in metabolic function brought about by surgical interference with the orderly function of the intestine and of the liver. He interprets these changes as due to some toxic substance produced or released by the liver as a result of the operation.

Experiments were conducted with human patients in an attempt to isolate possible toxic substances in the urine of the catatonic patient. He found that a substance which was closely allied to nicotine did produce catatonic symptoms in animals. However, it was not specific to catatonic patients, being recovered from the urine of certain normal humans. He then conducted a long series of experiments working through groups of chemicals more or less allied to nicotine and to histamine. He was unable to show that any one of these substances appeared in greater concentration in the blood of the catatonic patient than it did in his normal control group.

He sums up the methods by which experimental catatonia may be produced as follows:

- a. *chemical*: bulbo-capnine, cannabis indica, mescaline, substances preceding mescaline-synthesis, certain gases, substances derived from adrenaline, mescaline and choline, nicotine;
- b. *biochemical*: epinephrine, acetylcholine, asphyxiation, substances from human urine, benzoic urinary extracts;
- c. *alteration of metabolic function*: ligation of the hepatic artery, ligations of the small intestine, Eck's fistula;
- d. *electrical*: Leduc's current, faradic current, electroshock;
- e. *neuro-surgical*: various brain-lesions;
- f. *miscellaneous*: bilateral ligation of the carotid artery, centrifugation, audiogenic stimuli, "animal hypnosis" (only hypokinetic signs).

In light of this evidence he concludes that biologically human catatonia is in all probability closely related to some alteration of the function of the liver and the intestine which results in some toxic action on the brain cortex. This conclusion is in close agreement to the hypothesis advanced over fifty years ago by Kraepelin.

This is a report of a well-defined, scientific approach, in which the problem is stated and the resulting hypothetical deductions are experimentally tested. Not all of the experiments are adequately described, nor are all of them conclusive. However, the bulk of the work,

together with the unity of the evidence, is impressive. He has been able to show a great degree of similarity between human catatonia and experimental catatonia. He has been unable to specify the basic biological or physiological mechanism which produces the catatonic symptoms. He is to be commended for the thoroughness of his experimental approach. One might venture a prediction that the solution of the problem of schizophrenia will be found by the systematic continuation of the experimental approach which DeJong has so conscientiously followed.

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HARROWER, ERICKSON, M. R. & STEINER, M. E. *Large scale Rorschach techniques. A manual for the Group Rorschach and Multiple Choice Test.* Springfield, Ill.: Charles C Thomas, 1945. Pp. xi+419.

The late Max Wertheimer once described the relationship between his systematic position and that of Kohler and Lewin as follows: "Kohler is my son and Lewin is my nephew." A similar relationship may be said to exist between the individual Rorschach Method and the two techniques dealt with in this book: *The Group Rorschach* is a son whose claim to legitimacy is still in doubt, and the *Multiple Choice Test* is a second cousin. That it is necessary to make clear the distinction between the techniques is evident from the fact that in a recent standard textbook on abnormal psychology, the Group Rorschach and the Multiple Choice Test are confused with each other. Furthermore, the results from a study using the latter apparently are employed in drawing a conclusion as to the Rorschach Method.

This book purports to be a manual for the Group Rorschach (a technique of administering the Rorschach ink-blot to groups) and for the Multiple Choice Test (a technique in which the subject selects his responses to the Rorschach ink-blot from a list). It also presents certain normative material as well as the results of studies dealing with specific aspects of both techniques.

The material on the Group Rorschach is frankly designed for the use of persons skilled in the Rorschach Method. The literature on the Group Rorschach is well summarized, and a convincing case for the usefulness and merit of this approach is made. The authors contend on the basis of one study and a passing reference to two others that the group method yields essentially the same material as that yielded by the individual Rorschach. According to Dr. Marguerite Hertz, however, other studies not cited by the authors do not bear out this contention.\*

\* This point is made by Dr. Hertz in her review of the book in the *Rorschach Research Exchange* for March 1945. The entire review is recommended for those with a technical interest in the Rorschach Method.

In the section serving as a manual for the Group Rorschach, excellent directions for administration are given. The recommended methods for conducting the inquiry for determinants involve, however, what is tantamount to the use of leading questions—a practice which, for the individual Rorschach, both Beck and Klopfer strongly condemn. The chapter on scoring is not intended to aid the prospective scorer of the Group Rorschach since the authors believe that the scoring principles of the individual Rorschach apply. It seems likely, however, that all "F-" scores might not be the same with the two techniques and one might infer even from the authors' own data that "popular" responses and probably "usual detail" responses do not correspond entirely with their counterparts in the individual Rorschach.

Over 200 pages are devoted to data concerning locations, determinants, and content—mostly in graphic and tabular form. Unfortunately the value of this material is seriously weakened by the following major and minor flaws.

1. No account of the procedure of administration of scoring is given.
2. The sampling is inadequate. Of the 340 subjects tested, 224 were of college age, and the rest consisted of relatively small groups of adult males, prisoners, and a mixed group of psychotics and psychopathic personalities. It is very doubtful whether any meaningful differences can be found between groups so heterogeneous within themselves.
3. No measures of dispersion or measures of reliability of differences are presented.
4. The authors state that their "main interest and concern [was in] providing norms for the college age group" (4), but since their group is composed mainly of medical students and nurses, even this aim is not fulfilled.
5. Instead of attempting to ascertain what are the "usual details" from their own data, the authors use Klopfer's series and add other details which were seen by their subjects at least as often as the most infrequent one he lists. This results in their identifying as *usual* a large number of details whose actual frequencies are less than 1%, i.e., *unusual* "usual details."
6. Although average figures for the different determinants would have been helpful in interpreting a Rorschach record, only percentages are given.

On the jacket flap, the following description of the Multiple Choice Test is offered: "... by utilizing their research derived from large scale investigations they have devised an entirely new test which can be readily used for screening out maladjusted persons by investigation *without extensive training in the Rorschach Method.*" Although the original validating study of the authors showed a very good discrimination between normals and abnormals, subsequent studies (one of which is presented in full in the book) have found such imperfect discrimination as to make its value as a screening instrument very dubious. The authors attempt to explain away these findings, but the more recent study of Jensen and Rotter showing that the test places 45% of "actually excellent [Army] officers" in the abnormal group makes such attempts rather futile.

In an effort to overcome some of the shortcomings of the test, the authors developed an amplified form. In this version, the number of items was tripled, objectivity of scoring was increased, differential weighting was introduced and a new critical score adopted. These changes were made on the basis of an analysis of "more than 50,000 test blanks," but only fragmentary data are presented to justify most of the innovations. The reliability and validity of the new instrument are not mentioned. Fortunately a recent study allows a tentative evaluation of this form. R. F. and D. I. Malamud, comparing the responses of 100 normals with those of 100 abnormals, found an almost complete overlapping of the two distributions. Item analysis, with the standard of C.R. of 2 or over, revealed only 10% of the items as discriminative while 6% discriminated in the wrong direction.

Some material of value to those interested in experimental work can be gleaned from the book, but these hardly compensate for the many minor inaccuracies, the unsupported speculations, the overly optimistic attitude of the authors, the lack of organization of the material and the unduly low scientific standards employed. Since it is clear that both the Group Rorschach and the Multiple Choice Test are still in the experimental stage, the book offers little to the clinician.

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ROUSSEAU, JACQUES. *L'hérédité et l'homme*. Montreal: l'Arbre, 1945. Pp. 250.

This volume is published by France Forever, a Canadian affiliate of the French Committee for National Liberation, as part of an effort to keep French science alive. The author, who has written three previous books on genetics, appears to have aimed this volume for popular consumption, writing in simple witty style, and ranging from descriptions of the mechanisms of heredity to how to bring up children.

When the author writes about genetics no fault can be found, but when he crosses to psychology he appears to be less scientific and more vulnerable. He believes that nomadism and alcoholism are inherited factors, states that the existence of the criminally-born is definitely proven and after discussing the nature-nurture controversy states: "It is natural to conclude that the IQ is fixed by heredity."

A few incautious statements mar an otherwise excellent book. Altho an adequate glossary and index are provided, the bibliography is fragmentary and the author annoyingly does not further identify people whom he quotes.

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